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JULY TO DECEMBER, 1821.

VOL. II.

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ERRATA.

In Plate IX, fig. 3, *for* S, *read* P.

Page 219, line 11, *for* North Virginea, *read* Nerita virginea.

220, line 32, *for* vol. x. to xxiv. *read* vol. x. tab. xxiv.

220, *for* Rev. Dr. Daubeny, *read* Charles Daubeny, MD.

364, line 4, *for* By his, *read* By this.

365, end of Prop. VII. *for* arcs, *read* areas.

374, line 20, *for* $\frac{t}{t\,\tau'}(T\,\tau - t\,E)$, *read* $\frac{t}{t\,\tau'}(T\,\tau - T'\,E)$.

383, line 2, *for* world, *read* wind.

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ANNALS

OF

PHILOSOPHY.

JULY, 1821.

ARTICLE I.

A Memoir on the Physiology of the Egg, read before the Linnean Society of London, on March 21, 1809; *an Abstract of which is published in the Society's Transactions.* By John Ayrton Paris, MD. &c. &c.

(*Concluded from* p. 359, vol. i. *New Series.*)

THE part of the egg which next claims our notice is the air-bag, placed at its obtuse extremity; this follicle excited in me considerable interest, and was in fact the phenomenon that first directed my attention to the subject of the physiology of the egg; and when I found that this organ had not received the attention which its importance seemed to merit, I was encouraged to pursue its investigation. I am not aware that it exists in the ova of any animals but those of birds: indeed there is a wonderful relation between the respiration of oviparous animals before and after their exclusion from the egg; so, perhaps, birds who enjoy the most perfect species of respiration, are those only whose egg contains a particular organ for the purpose.

The external shell and the internal membrane with which it is lined, constitute the parietes of the *folliculus aëris;* its extent in the recent egg is extremely small, and before its exclusion from the uterus, it does not appear to exist; it would seem to commence at the moment the egg is deposited by the bird; a small portion of the watery contents of the egg transpire through the shell, and the air then rushes through the obtuse end and inflates the follicle: this is the history of its origin; and its size and subsequent increase are to be explained upon the same principle, which thus establishes an important relation between the diminution of the bulk of the ovular contents and the extent of this pneumatic apparatus. During the progress of incubation, it is dilated to a very considerable magnitude. Its uses seem to have been first understood and appreciated by Harvey: "*Utilis est ad ovi ventilationem, ac Pulli perspirationem, refrigerium, et respirationem, ac denique ad loquelam, unde cavitas illa primo exigua, mox major, ac denique maxima conspicitur, prout varii jam nempe dicti usus postulaverint.*"

Very early after incubation, the *cicatricula* expands into several circles, containing an ash-coloured fluid, called by Harvey "*colliquamentum*" in this, on the fourth day, the heart, like a vibrating point, "*punctum saliens,*" for the first time, becomes visible, and blood-vessels are seen defining, like a fringe, the *cicatricula;* these *meatus venales,* which are hereafter to become the *umbilical* vessels, extend and multiply their ramifications on the yelk and white, by which the blood is exposed to the action of the air in the follicle, oxygenated, and returned to the embryon: to establish, however, this theory upon a solid basis, it became necessary to discover the nature of the air that inflates the follicle, and which has hitherto remained unexamined. We are informed by Buffon, that it is a product of the fermentation which the different parts of the egg undergo. If the Count's conjecture be substantiated, the gas must be *non-respirable:* to determine this point, and to discover whether the process of incubation produces any change in its chemical constitution, I instituted the following experiments:

Experiment 1.—Twenty-one hens' eggs newly laid, when broken under the surface of water, yielded only one cubical inch of gas: this, when received in a jar, and subjected to an eudiometric test, proved to be pure atmospheric air.

Experiment 2.—Two eggs, after 20 days' incubation, were opened as before, when one cubic inch of gas was collected, which I also discovered to be atmospheric air, contaminated, however, with a portion of *carbonic acid.* This latter gas I suspect to be derived from the *venous* blood of the chick, which seems to establish another analogy between this mode of oxygenation and respiration after birth.[*] From these results, the following corollaries may be drawn: 1. The *folliculus aëris* contains before incubation atmospheric air.[†] 2. No other chemical change is effected in its constitution than a small inquination with carbonic acid. 3. It gains by incubation an increase of volume, which takes place nearly in the ratio of 10 to 1; it must, however, be remarked that its extent does not increase equally in equal successive portions of time; but that it observes a rate of progression which is accelerated as the stages of incubation advance, although it seems to arrive at its *maximum* of dilatation a few days previous to the exclusion of the animal.

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[*] As in respiration, may not this combination of oxygen with the blood generate heat? For Mr. Hunter's experiments prove that there is a difference of several degrees in the temperature of an addled egg, and in that of one advanced in its evolution, although they have both been alike subjected to the animal heat of incubation.

[†] By the application of heat this air is expanded, and, if suddenly, it will burst the shell, and scatter the contents; the obtuse extremity should, therefore, be always pricked with a pin before the egg is roasted, a fact well known to the country housewife; and hence the old adage, "*There is reason in roasting an egg.*"

Some naturalists have conjectured that the use of this apparatus is to furnish the air with which the feathers are inflated: this idea hardly requires a serious refutation; we detect the same receptacle in the eggs of those birds that are hatched unfledged. Its essential purpose is undoubtedly to *oxygenate* the blood of the embryon, and we accordingly find that whatever obstructs the inflation of this follicle, and the renewal of its air, destroys the life of the chick. The experiments of Reaumur offer abundant proofs of this truth. In his attempts to develope the egg by the heat of dung, they, for a long time failed, owing to a circumstance which he afterwards discovered to depend upon the impurity of the atmosphere. He also varnished eggs so as to prevent the access of air; and he found that when placed under the hen, they invariably perished. Spallanzani instituted many experiments with the same view. "I have often," says he, "placed the eggs of terrestrial and aquatic insects under the receiver of an air-pump, but none ever hatched in this situation, although in every other respect in a condition to have done so;" and Boerhaave offers his testimony upon the same subject in the following words: "*Ovula quorumcunque insectorum, in vitris accurate clausis, non producunt.*" We see the importance, therefore, of that provision by which the egg is occasionally ventilated by the migration of the parent; it is a fact well known in the farm yard, that turkeys frequently destroy or smother their eggs by a too constant and assiduous attention.

The *air follicle* may also have a secondary office to perform, to assist in producing necessary changes in the *albumen* and *vitellus* by the chemical action of its air. Such then is the nature of this organ in the egg of the common hen, from which this description is taken; the same apparatus exists in the eggs of all birds, and contains a similar air; its capacity, however, does not seem to vary either with the size of the egg, or of the bird to which it belongs; but I think that I have discovered a beautiful law by which its extent is regulated. I have uniformly found (as far as my contracted inquiry has led me), that the *folliculus aëris* is of greater magnitude in the eggs of those birds who place their nests on the ground, and whose young are hatched, fledged, and capable of exerting their muscles as soon as they burst from their shell, than the *folliculi* of those whose nests are generally built on trees, and whose progeny are born blind and forlorn. Thus the *folliculi* of the eggs of hens, partridges, and moor-hens, are of considerable extent; while those of the eggs of crows, sparrows, and doves, are extremely contracted. The chick, therefore, of hens and partridges has a more perfect plumage, and a greater aptitude to locomotion, than the callow nestlings of crows and sparrows.

Such an instance of the agency of *oxygenation* in the promotion and increase of muscular power is not solitary; the history of ruminating animals will furnish a parallel example. "Their cotyledons," says the ingenious author of the Zoonomia, "seem to be designed for the purpose of expanding a greater surface for the termination of the *placental* vessels, in order to receive *oxygenation* from the *uterine* ones: thus the progeny of this class of animals are more completely formed before their nativity than that of the carnivorous classes; calves, therefore, and lambs, can walk about in a few minutes after their birth; while kittens and puppies remain many days without opening their eyes." In confirmation of the theory, that *muscular irritability is the result of a nice combination of oxygen with the animal organs*, many interesting facts may be adduced. Do we not find that the muscular strength of an animal is (*cæteris paribus*) proportional to the extent[*] and perfection of its respiration? Birds are enabled to sustain the exertion of flight, owing to their extensive pneumatic receptacle; and many insects, especially the different species of *Scarabæus*, in the act of flying, disclose avenues of air, which in their quiet state are closed by the cases of their wings, thus procuring for themselves a larger supply of the principle of muscular energy at a period when from their exertion, and consequent exhaustion, they most require it: flat fish, who having no swimming bladder, remain at the bottom, and possess but little velocity, have gills that are quite concealed, while those who encounter a rude and boisterous stream, as trout, perch, or salmon, have them widely expanded; and, with respect to the respiration of fishes, it may be further observed, that *the sum of oxygen which they receive will vary jointly as the momentum of the water which imparts it, and the extent of the gills.* An acquaintance with this truth at once enables us to discover one of the most beautiful final causes in nature. We shall no longer consider the rapid current or the boisterous ocean as inimical to the strength of the animal, but regard them as the powerful causes of its invigoration—"*Elementa propriis armis devicta.*" So also the velocity of fishes, and its unwearying duration, will cease to astonish us, since it is evident that such motions contribute as well to the revival as to the exhaustion of muscular energy; for although they must waste the stream of irritability, yet they add to the fountain by which it is supplied. Hence it follows, that whenever it is an object to economize the consumption of air, as must happen in crowded and confined situations, we ought to preserve our muscles, as far as we are able, in a state of complete inactivity; nothing could have been more judicious than the advice given by Mr. Holwell to his unfortunate fellow sufferers in the Black Hole at Calcutta, "*not to exhaust their strength by useless efforts, but to remain quiet and orderly,*" or in other words, not to waste, wantonly, the animating principle of the atmosphere by motions, the effect of which is to render an increased consumption of it unavoidable: on the contrary the expedients resorted to upon that melancholy occasion, were calculated to accelerate the fatal result which they were intended to counteract, such as fanning the air with their hats, and kneeling down, for the purpose of simultaneously rising, in order to give a fresh impulse to their stagnant atmosphere.

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[*] Narrow-shouldered men bear labour worse and pain better than others. Thus the natives of North America, a narrow-shouldered race of people, will rather expire under the lash than be made to labour.—(Darwin's Zoonom. vol. ii. p. 14.)

It is evident that the inflation of the *folliculus aëris* of the egg will proceed in the same ratio as the evaporation of its fluid contents: the importance of such an arrangement is sufficiently obvious. I shall, therefore, proceed to describe the shell, the only part of the egg which now remains to be noticed. The use of the shell is not only to defend the ovular structure from external violence, but to regulate the evaporation of the fluid contents, and the various chemical changes essential to its developement. It consists, according to the latest experiments of M. Murat Guillot,* of gelatine 3, phosphate of lime 2, carbonate of lime 72 parts, united to an organic tissue. Mr. Carlisle, in a memoir read before the Royal Society, "Upon the Vascular and Extra-vascular Parts of Animals," states, that the calcareous shells of birds' eggs are merely deposited upon the *membrana putaminis*, and that the inner portions are regular crystallized prisms, the long diameters of which point to the centre of the egg. I have repeatedly endeavoured to discover such a crystalline structure, but unsuccessfully; and my friend Mr. William Phillips, whose knowledge of crystallography is so justly appreciated, has examined the egg shell with no better success. The hard and brittle texture of the shell is increased by incubation, and it also undergoes some other changes during this period which are not well understood.

Every circumstance connected with incubation discovers an evident design to conceal the bird and its egg: the hen in general is not dressed in the gaudy plumage which distinguishes the male, nor is she endowed with the talent of singing, lest her note should arouse the vigilance of her enemies: so the colours of the egg vary in the different species of birds, and seem well adapted to the purpose of concealment. "Thus," says Dr. Darwin, "the eggs of hedge birds are greenish with dark spots; those of crows and magpies, which are seen from beneath through wicker nests, are white, with dark spots; and those of larks and partridges are russet and brown, like their nests and situations."

The matter from which the shell is formed is secreted in the lower portion of the uterus, and in this operation we recognise a process which, at the same time, answers two of the most important purposes of the animal; it at once serves the individual, and contributes to the perpetuation of the species; for while it removes the superabundant calcareous matter, which, if allowed to accumulate, must render the bird incapable of flight, and defeat the best purposes of its existence, it furnishes the germ of the future animal with a strong and convenient defence. It sometimes happens that the eggs of birds are deposited without the shell; this may arise from the secretion of calcareous matter not keeping pace with the too exuberant production of the yelks, a circumstance which may depend upon a variety of causes; but as it is not my intention to discuss the question of the origin of lime in animal bodies, I shall, upon the present occasion, rest satisfied with recording some facts connected with the subject. The experiments of Vauquelin, which prove that the quantity of calcareous matter voided by the system exceeds that taken in with the food, suggested to Fordyce that birds must require calcareous matter during their laying, and that if the animal were deprived of this earth, the shell would never be formed. From observations made by myself, I am inclined to reject this theory; for birds occasionally deposit eggs without shells, who have free access to lime; and, on the other hand, although they be carefully kept from lime, they will nevertheless produce calcareous secretions.

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* Annales de Chimie, vol. xxxiv.

As far as the light of analogy extends, it would seem that lime is a product of animalisation, and that its secretion requires a considerable energy of constitution; this is rendered probable by the well-known phænomena of *rachitis;* the absence of the shell, therefore, depends probably upon some constitutional cause in the bird, and not upon the privation of lime.

During my experiments, many years since, a curious circumstance occurred to a hen that was kept for the purpose that deserves to be placed upon record. This bird had broken its leg, and the limb was carefully bandaged, when, after a few days, several eggs destitute of shells were found upon the premises, which I ascertained had been produced by the bird in question. Now it may be fairly asked whether, in this case, the calcareous matter designed for the formation of the shell was not employed in the regeneration of bone? In the human species, the converse of this takes place: for a fracture, occurring during pregnancy, frequently does not unite until after delivery.* Here then nature evinces a greater anxiety for the offspring than for the parent; while, on the contrary, the fecundity of an oviparous animal would seem to render such a precaution unnecessary. The same law will explain why women who have had many children in rapid succession occasionally become rickety, and are affected with a species of *mollities ossium;* the fœtus derives its whole supply of ossific matter from the mother, and if she be exhausted, sick, or ill fed, this would appear to be done at an expense which her own bones cannot bear without injury. I shall terminate this subject by observing that the deer are incapable of procreating their species, if their horns be broken at the *rutting* season.

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* In the fourth volume of Medical Observations and Inquiries, a case is communicated by Dr. William Hunter from Mr. Alanson, Surgeon at Liverpool, of a simple fracture of the tibia in a pregnant woman, where the callus was not formed until after delivery.' The accident happened during the second month of her pregnancy, and until after delivery no adhesion had taken place; but in the course of nine weeks afterwards

ARTICLE II.

Researches on the Composition of the Prussiates, or ferruginous Hydrocyanates. By J. Berzelius.

(*Concluded from* p. 449, vol. i. *New Series.*)

I THINK that the following conclusions may be drawn from the experiments already detailed, viz.

(*a.*) That the cyanurets of the alkalifiable metals preserve their cyanogen at a very high temperature, but that the cyanuret of iron combined with them suffers decomposition, and gives azotic gas, and leaves quadricarburet of iron.

(*b.*) The cyanurets of other irreducible metals are decomposed at a high temperature. Those which can be entirely deprived of water, as the cyanuret of iron, give azotic gas, and are converted into a double quadricarburet. Those, on the contrary, which preserve their state of hydrocyanate until decomposition commences, lose a certain quantity of their carbon, and the carburet which remains contains the iron in the form of quadricarburet; but the other metal is carburetted in a less degree, being either a tricarburet or bicarburet.

(*c.*) Reducible metals lose the cyanogen with retaining the carbon; but it is probable that some among them may, at a higher temperature, divide the carbon with the carburet of iron. The compounds of carbon with the metals have hitherto but very little attracted the attention of chemists. It has been indeed found that the metals reduced by carbon always retain a small portion of this body, by which their properties are more or less altered. But the carbon thus absorbed by the metals is mostly in so small a quantity that its relation to chemical proportions cannot be determined. We were ignorant until now of metallic carburets, which were proportional in composition to the sulphurets, the arseniurets, &c. and also to that of the carbonates. It is evident that those which have been now described belong to this latter class of compounds; for the decomposition

she was able to walk about the room. There are also three cases in *Hildanus* of fractures which took place in pregnant women, where a union could not be procured by a continuity of bony callus. In Heister's Surgery, the reader will find several other similar cases.

of the cyanurets by heat is entirely determined by the affinity of carbon for the metals; if it were otherwise, the cyanogen would either remain combined with them, as, for example, with the alkalifiable metals, or would separate without decomposition, as occurs with the metals reducible *per se*.

In the foregoing experiments, we have not only seen bi, tri, and quadricarburets, but we have also discovered double carburets analogous to double sulphurets and arseniurets, numerous examples of which are met with in the interior of our globe. The existence of these carburets depends probably upon the same affinity as that which gives rise to double cyanurets.

In distilling vegetable salts with a metallic base, the metallic carburets are also obtained, and which have been in general considered as mixtures of carbon and metal; but it is very certain that in a great number of cases, if not in all, these residues are metallic carburets of determinate composition, and that the quantity of carbon which is found in the volatile products of the distillation is in part determined by the affinity of the metal for this element.

The phenomenon of combustion which is observed in the greater number of the above-mentioned experiments is an interesting addition to those which have been before presented by some metallic antimoniates, gadolinite, hydrate of zircon, as well as the oxides of chrome, rhodium, and iron. This combustion appears to arise from a more intimate combination between the iron and the carbon than that which existed in the cyanuret. Every sudden rise of temperature which occurs in these bodies consequently appears in the form of an eruption of fire.

For those who have not yet attended to this singular phenomenon, I will here relate an experiment which is very easily repeated. A solution of sulphate or muriate of deutoxide of iron is to be decomposed by ammonia added slightly in excess, in order to prevent the oxide of iron from carrying down any acid in the form of subsalt. The oxide is to be well washed and dried. It is afterwards to be slowly heated in a small platina crucible, by exposure to the flame of a good spirit lamp, until the crucible begins to become red, and the water and ammonia consequently driven off. The fire is then suddenly increased to redness; the pieces of oxide of iron begin to increase in volume; slight motion is perceived here and there; all at once they take fire, and intense ignition pervades them from one end to the other. The oxide neither gains nor loses any thing by this phenomenon; and if there is any change of weight, it is always diminution, occasioned by not having left it long enough exposed to a moderate heat, to expel all the water and ammonia which it had retained. After having undergone this apparent combustion, the oxide of iron is rendered more difficulty soluble in acids; it is dissolved, nevertheless, by continued digestion in concentrated muriatic acid; but if it be precipitated again, fire is reproduced under similar circumstances.

I have already spoken in another place * of this phenomenon and of the conjectures which may be made as to its cause. If, at the time of decomposing some cyanurets, the temperature is too high, the phenomenon of combustion would coincide with the disengagement of azotic gas; and it would appear that one ought not to occur without the other, as we know with respect to the superoxide of hydrogen, and also with the muriatic radical (oxide of chlorine), that combustion and oxygen gas are produced together at the same instant. With the cyanurets, this phenomenon is divided into two periods, as we have seen—that of the decomposition of the cyanuret, and of the ignition of the remaining carburet of iron. This circumstance gives fresh support to the opinion, that, as happens with the superoxides, the igniferous decomposition is composed of two operations, one of which consists in the disengagement of a portion of oxygen, and the other in the more intimate union with that which remains, and that consequently any exception to our general experience, as that the separation of two simple bodies should be accompanied by ignition, as happens in the combination of the stronger elements, ought not to exist.

VI. *On the Combination of Cyanurets with Sulphuric Acids.*

The cyanurets when treated with concentrated sulphuric acid are more or less dissolved without being decomposed. Those of iron and potash, of iron and barium, dissolve without any residuum, and yield colourless solutions which sustain a heat much above 212° Fahr. without decomposition. Some others alter in appearance, a very small quantity being dissolved by sulphuric acid, the greater part remaining undissolved by it, and possessing new properties. When sulphuric acid is poured upon a cyanuret reduced to powder, the mixture frequently becomes considerably hot; the cyanuret increases in volume, becomes pulpy; and, if it is soluble in the acid, it disappears gradually, and the mixture becomes liquid. It is requisite to have a great excess of sulphuric acid to dissolve even the most soluble. If to this solution a little water be added, it becomes turbid, and deposits a part of that which it held in solution; that is to say, a part of the compound of the cyanuret with sulphuric acid. If much water be added at once, the compound of the cyanuret with the acid is decomposed, and superhydrocyanate of iron, and a supersulphate of the other base are obtained; or if the cyanuret is insoluble, it reappears with its usual characters.

If the acid solution of a cyanuret be left exposed to the air, it gradually attracts moisture, and the new compound dissolved is proportionally deposited, sometimes in a crystalline, and at other times in a powdery form. On the other hand, if the solution of a cyanuret in sulphuric acid be heated, there is a certain temperature at which, when it arrives, it effervesces, the cyanuret is decomposed, and a great quantity of gas is disengaged with rapidity. This gas is a mixture of sulphurous acid gas, carbonic acid gas, and azote. The residual mass contains supersulphates of the bases employed and ammonia.

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* Essay on the Theory of Chemical Proportions, &c. Paris, 1819. Page 81.

Dr. Thomson asserts that on this occasion a new gas is formed, and that this gas is composed of hydrogen, carbon, and oxygen, the proportions of which he has apparently determined with care. He has moreover given the specific gravity of this gas, and determined the condensation of its elements at the moment of their union. I have repeated the experiments of Dr. Thomson, according to the directions which he has given, and which are rather indefinite. Of the gas which I obtained, 0·348 part was absorbed by the black superoxide of lead, and 0·25 part by caustic potash. The remainder, which contained the atmospheric air of the apparatus before the commencement of the operation, and the azotic gas which was not converted into ammonia, were not inflamed by the electric spark, neither unmixed or mixed with oxygen gas in different proportions. Lime-water was not rendered at all turbid, and even when I added hydrogen gas and inflamed the mixed gas, lime-water remained clear. I decomposed in the same mode the crystalline compound of sulphuric acid and cyanuret of potassium and iron, and it gave me the same products. The action of fire immediately decomposes it with rapidity, and afterwards a slower disengagement of gas takes place: which happens when the sulphate of ammonia formed decomposes. The gases which are then evolved are sulphurous acid and azote. But even on this occasion no combustible gas was perceptible.

I am now going to give a particular description of the compounds of some cyanurets with sulphuric acid.

1. *Cyanuret of Iron and Potassium with Sulphuric Acid.*—If the acid be added to the anhydrous cyanuret, it becomes very hot, and if the acid be in sufficient quantity, the cyanuret is totally dissolved, and after digesting for a few minutes, it gives a clear and colourless solution. When left in an open vessel for some days, the mass becomes pulpy, and filled with numerous small annular crystals, surrounded with sulphuric acid less concentrated. I took this mass after eight days' exposure to the air, and put it upon a brick, and placed it in vacuo, in order to avoid the influence of atmospheric moisture upon the crystallized part, and to facilitate the absorption of the liquid part. At the expiration of 24 hours, I found upon the brick a white crystalline mass composed of small interlaced acicular crystals. This mass is soluble in water, and the solution has all the properties of one of supersulphate of potash mixed with that of superhydrocyanate of iron, which does not decompose by exposure to air, as it would have done alone. Even alcohol of 0·81 density decomposes this salt, combining with the hydrocyanate of iron and the sulphuric acid, and leaving the sulphate of potash. I analyzed this compound in the following manner: I decomposed it by

alcohol: the undissolved salt was washed with alcohol, containing a very little caustic ammonia, in order to satisfy myself that the undissolved sulphate of potash was neutral. The alcoholic liquors were afterwards diluted with water and precipitated by muriate of barytes. I obtained nine parts of sulphate of potash and 40 parts of sulphate of barytes. An experiment of this nature cannot be very exact; for the substance to be analyzed always contains some sulphuric acid on the surface. Thus in this analysis the sulphate of barytes contains rather more than three times as much sulphuric acid as the sulphate of potash. Considering the excess of sulphuric acid as adhering to the surface of the crystals, it follows that the cyanuret of iron and potassium must have been combined with a sufficient quantity of sulphuric acid to form a bisulphate with the potash, and common sulphate with the protoxide of iron.

2. *Cyanuret of Iron and Barium with Sulphuric Acid.*—This compound is much less soluble in sulphuric acid than the foregoing. It crystallizes readily when made to attract moisture from the air. The crystallized salt is decomposed by water and by alcohol, and gives sulphate of barytes, hydrocyanate of iron, and sulphuric acid. An analytic experiment, but which is less deserving of confidence even than the foregoing, because the crystals of the barytic compound are smaller, and consequently retain more sulphuric acid, gave me a quantity of sulphuric acid necessary to form bisulphates both with the barytes and the protoxide of iron.

3. *Cyanuret of Iron and Lead* was strongly heated in sulphuric acid, but the new compound is almost insoluble. The sulphuric acid added in excess is rendered slightly turbid by the addition of a little water; but I observed no traces of crystallization even after many weeks' exposure to the air.

4. *Cyanuret of Iron and Cobalt* readily dissolves in, and gives a red colour to, sulphuric acid. After some hours, the liquid deposits a crystalline powder of a very fine rose colour, and loses at the same time much of its colour. I thought at first that the rose powder might be sulphate of cobalt; but when water is added to it, it becomes at first green, and afterwards, in proportion to the action of the water, it assumes the reddish-grey colour of hydrocyanate of cobalt. In order to explain what happens in this experiment, I ought to add, that when a solution of cobalt is poured into one of cyanuret of iron and potassium, there is at first formed a green precipitate which gradually becomes of a reddish-grey colour. If it be dried and then heated, it yields water and a little ammonia combined with carbonic and hydrocyanic acid, and resumes its original green colour. The changes of colour, which resemble those that occur with the muriate of cobalt, appear to depend on water, which, when the green colour of the anhydrous cyanuret changes and becomes reddish-grey, is absorbed, whether it forms an hydrocyanate, or produces water of crystallization. As then

the red compound of sulphuric acid with cyanuret of iron and cobalt becomes green by the addition of water, it seizes the sulphuric acid, and the moment afterwards the cyanuret set at liberty becomes hydrated. The solution of this cyanuret in sulphuric acid does not give crystals by exposure to the air; it has a dirty-red colour, and water precipitates hydrated cyanuret. The diluted solution contains cobalt.

5. *Hydrocyanate of Iron and Deutoxide of Copper* loses its brown colour when it is mixed with sulphuric acid, and becomes white with a shade of greenish-yellow. It is very little soluble in sulphuric acid; water decomposes this compound, and the hydrocyanate of iron and copper reappears with its original colour, without any copper being dissolved in the diluted acid.

6. *Prussian Blue* increases in volume in sulphuric acid, becomes white, and resembles starch. The new compound is insoluble in excess of acid. If the prussian blue of commerce is made use of, the acid becomes brown, or even black, by carbonizing the foreign substances which it contains. The acidulous mass, when dried upon an absorbing brick, leaves a pulverulent white substance, which does not exhibit any appearance of crystallization. When mixed in a close vessel with water deprived of air, it is immediately decomposed, and resumes its blue colour, the water combining with the sulphuric acid. This experiment proves that the sulphuric acid combines with the hydrocyanate without converting it into a cyanuret; for, in this case, the water would have separated white cyanuret of iron, and dissolved sulphate of deutoxide, and would not have separated prussian blue.

7. *Cyanuret of Iron and Silver* is also soluble in sulphuric acid, but it is decomposed in part at least. A yellowish substance remains undissolved. The colourless liquid, exposed to air and the sun, does not become black, and deposits small crystalline grains of sulphate of silver.

8. *Cyanuret of Mercury* gives with sulphuric acid a mass resembling starch. A slight smell of hydrocyanic acid is perceived, and if much sulphuric acid be added, it becomes of a yellowish colour, probably on account of the decomposition of a small portion of hydrocyanic acid. The acid dissolves very little of the new compound; by the addition of water a small quantity is deposited, which, by adding more, is dissolved. In the same way, the insoluble compound, when treated with water, is dissolved without leaving any residuum.

It remains for me now to say a few words on the nature of the compounds of sulphuric acid with the cyanurets. They may be considered in two modes, either as sulphates of cyanurets, in which the cyanogen acts as oxygen in the base, or as supersalts with two bases and two acids. My first idea in observing these compounds was, that the cyanurets might be considered as oxygenated bodies, either acids or bases, which could combine not only with each other, but with the oxygenated acids. We have several double cyanurets of iron, copper, silver, and gold, with other metals, and it appeared to me very probable, that the cyanuret of the electro-negative metal might act as an acid with respect to the electro-positive metal, which represents the base. The sulphuretted hydrocyanic acid might very well be a double cyanuret of sulphur and hydrogen; and in this way the theory of these compounds became simple and analogous to that of the oxides. I afterwards examined the sulphuretted hydrocyanic acid, and I found that its nature was altogether different; and I shall soon have occasion to mention it to the Academy. As to the idea of regarding the compounds with sulphuric acid as sulphates of cyanurets, the experiment with prussian blue is opposed to it; and that which I am going to relate immediately, decides, as it appears, the question in the negative. I took some hydrocyanate of iron which had been dried in vacuo, and I treated it with sulphuric acid. It dissolved, and produced a colourless, and limpid liquid, which gave no trace of hydrocyanic smell. When exposed to the air, the sulphuric acid deposited a white substance, which I separated from the acid liquor, by means of an absorbing brick. The compound of sulphuric acid with the superhydrocyanate of iron remains in the form of an uncrystalline powder. It is totally soluble in water, which afterwards contains sulphuric acid and superhydrocyanate of iron, which decomposes by exposure to the air, as if no sulphuric acid were present: this then is a proof that water has separated them. As in this compound no doubt can exist of the sulphuric acid being combined with hydrocyanic acid, it appears to me to be decided, that the compounds of which we have been speaking are in reality acidulous double salts, in which two bases are combined with an excess of two acids.

VII. *Observations on the Preparation of the Alkaline Cyanurets by Means of Prussian Blue.*

If the prussian blue of commerce be treated with caustic potash in excess, we obtain, after the crystallization of cyanuret of iron and potassium, a syrupy mother-water, which refuses to crystallize, but which, when slowly evaporated, effloresces in greenish vegetations. If the excess of potash in the mother-water be neutralized with acetic acid, and if alcohol be afterwards added, it separates a mass of a deep green colour. It is a peculiar modification of cyanuret of iron and potassium, which dissolves in water, and gives a meadow-green colour, but by long exposure to moist air, it becomes brownish. It does not crystallize by evaporation, but it deposits small green scales, especially at the edges of the liquid. The colour of these scales becomes paler, and brownish when they are dried. I analyzed them, and they differed so little from the yellow cyanuret that I could draw no conclusions from the analysis. These differences are derived from a peculiar modification of cyanogen, which exists in prussian blue. While decomposing, this cyanuret deposits a green powder, the quantity of which increases during evaporation; and by long exposure to the atmosphere, we procure at last crystals of the common cyanuret.

The best method of getting rid of this modification of cyanogen is to heat the anhydrous mass in a well-covered crucible until it begins to fuse. The crucible is then to be removed from the fire, the mass is to be suffered to cool, and then dissolved in water. Some charcoal and carburet of iron remain undissolved. The solution contains cyanuret of iron and potassium, hydrocyanate of potash, and carbonate of potash. Acetic acid is to be added to decompose these two last salts with base of potash, the liquid is concentrated, and the cyanuret of iron and potash precipitated by alcohol. It is to be afterwards crystallized, and it is then obtained of a constant light-yellow colour, and finer than in any other manner. In the common way it is always procured of a variable shade of colour.

If prussian blue be treated with sulphuric acid, or still better with muriatic, this modification of cyanogen is for the greater part avoided.

Hydrate of barytes also produces a green compound with prussian blue. The liquid loses its green colour; but it is restored by evaporation to dryness, and if the salt mixed with alcohol is exposed to the sun's rays in a stopped bottle. When mixed with a solution of deutoxide of iron, it gives prussian blue, exactly like the green cyanuret of iron and potassium. I examined this substance but very superficially.

Hydrate of lime gives scarcely any trace of similar combination; but it decomposes prussian blue very imperfectly; an insoluble mass of a light ochre colour is obtained, which suffers no further change by an excess of hydrate of lime, and which is a subhydrocyanate of lime and oxide of iron. Acids decompose it, combining with the lime, and separating the prussian blue. It is probable that the green modification remains insoluble in the subsalt.

Ammonia gives the green modification in great abundance; frequently nothing else is obtained. It crystallizes in the form of small green needles. Alcohol precipitates it of a green colour, but of the consistence of a syrup. The aqueous solution deposits a green powder during evaporation; it is sometimes possible to obtain some crystals of common hydrocyanate of ammonia and iron; but the greatest part decomposes by long exposure to the air, and gives a green powder.

Hydrocyanate of ammonia without iron suffers decomposition even in close vessels, and when surrounded by its own gas, it gives rise to a brown substance, which often preserves the outline of the crystals. After this decomposition, ammonia is obtained, which precipitates the salts of deutoxide of iron of a green colour. The brown mass does not possess this property.

The green powder which is obtained in all these experiments is the modification of the hydrocyanate of protoxide and deutox-

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ide of iron corresponding to those which have been examined with the other bases. It is not oxidated prussian blue, the blue colour of which is restored by reducing; but the sulphuric and muriatic acids restore it. Potash decomposes it, leaving a yellowish-green mass undissolved. This green powder gives much carbonate of ammonia and empyreumatic oil during decomposition.

Article III.

On an Alkalimeter and Acidimeter. By Dr. Ure.

(To the Editor of the *Annals of Philosophy.*)

SIR, *Glasgow, April* 15, 1821.

In page 13 of the Introduction to the Dictionary of Chemistry lately published, I have alluded to Dr. Henry in terms which have occasioned a private correspondence between that gentleman and me, the result of which we are desirous of making public in your journal.

In the beginning of August, 1816, I transmitted to him an Essay on Alkalimetry and Acidimetry, accompanied by a letter, in which I begged him to favour me with his opinion of its merits, cautioning him meanwhile not to communicate its contents to any person. In the eighth edition of his Elements, which appeared in 1818, he published a plan of alkalimetry and acidimetry modified from that described in my Essay. This struck me at the time as an unwarranted use of my communication; and declining to correspond with him on the subject, I resolved to seize the first favourable opportunity to reclaim my rights. Under this feeling I wrote the paragraph in the Introduction to the Dictionary.

Dr. Henry thus writes me on the 12th of April, 1821, "I assure you that I had not at the time of publishing my book, nor can I now recall, the remembrance of any injunction of secrecy, respecting your alkalimeter. I conceived I had so expressed myself, at p. 512, vol. ii. of my Elements, as unequivocally to give to you the credit of inventing an instrument on the principle of directly, and without calculation, indicating the per centage of alkali in any specimen; and that I pretend to nothing more than the modification of your method which is described in my book."*

Under these circumstances, I am satisfied that Dr. Henry had no intention to appropriate to himself the credit of my invention; but I sincerely regret that, before promulgating the modification of my method, he had not consulted me on the subject. This would have prevented all chance of misunderstanding between me and Dr. Henry, whose accomplishments as a gentleman and a chemist, I have been accustomed to admire. The readers of the Dictionary will perceive, under the articles Calculi, Coal Gas, Gas, Salt, &c. that I have not suffered temper to influence my judgment, but have done merited honour to the Doctor's researches on every scientific occasion.

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* "It has been very properly objected to it (the alkalimeter of Descroisilles) by Dr. Ure, of Glasgow (in an Essay on Alkalimetry, which he was so good, about two years ago, as to communicate to me in manuscript, and which, I believe, he has not yet published), that these degrees, being entirely arbitrary, do not denote the value of alkalies in language universally intelligible; and he has proposed an instrument which shall, at once, and without calculation, declare the true proportion of alkali in 100 parts of any specimen. The principal deviation in the following rules from the method of Dr. Ure, is," &c. &c.—(Henry's Elements of Chemistry, vol. ii. p. 51.)

I have the honour to be, Sir,
Your most obedient servant,
ANDREW URE.

ARTICLE IV.

On the Finite Values of Circulating Decimals.

By Mr. James Adams.

(To the Editor of the *Annals of Philosophy.*)

SIR, *Stonehouse, near Plymouth, May* 8, 1821.

Problem 1.—To find the sum of n terms of a geometrical progression.

Let A, B, C, D, E, be any series in continued proportion, then will A : B :: B : C :: C : D :: D : E; where A, B, C, D, or all the terms, except the last, are antecedents; and B, C, D, E, or all the terms, except the first, are consequents. Put S equal to the sum of all the terms, then will S − E = sum of all the antecedents, and S − A = sum of all the consequents, whence (12 . *e* . 5) A : B :: S − E : S − A; therefore, $A\,S - A^2 = B\,S - B\,E$, from hence $S = \frac{A^2 - B\,E}{A - B}$.

Corollary.—In a decreasing series continued *in infinitum,* the last term E vanishes, and the sum of the whole series is $S = \frac{A^2}{A - B}$.

Problem 2.—To find the finite value of any *pure* circulate.

$$\cdot\dot{3} = \frac{3}{10} + \frac{3}{100} + \frac{3}{1000} + \frac{3}{10000} + \&c. \quad \text{Or,}$$

$$\cdot\dot{3} = \frac{3}{10}\left\{1 + \frac{1}{10} + \left(\frac{1}{10}\right)^2 + \left(\frac{1}{10}\right)^3 + \&c.\right\}$$

$$\cdot\dot{5}\dot{8} = \frac{58}{10^2}\left\{1 + \left(\frac{1}{10}\right)^2 + \left(\frac{1}{10}\right)^4 + \left(\frac{1}{10}\right)^6 + \&c.\right\}$$

$$\cdot000\dot{4}\dot{6} = \frac{46}{10^5}\left\{1 + \left(\frac{1}{10}\right)^2 + \left(\frac{1}{10}\right)^4 + \left(\frac{1}{10}\right)^6 + \&c.\right\}$$

$$\cdot\dot{0}\dot{4} = \frac{4}{10^2}\left\{1 + \left(\frac{1}{10}\right)^2 + \left(\frac{1}{10}\right)^4 + \left(\frac{1}{10}\right)^6 + \&c.\right\}$$

$$\cdot\dot{6}7\dot{4} = \frac{674}{10^3}\left\{1 + \left(\frac{1}{10}\right)^3 + \left(\frac{1}{10}\right)^6 + \left(\frac{1}{10}\right)^9 + \&c.\right\}$$

Generally

$$S = \frac{R}{10^n}\left\{1 + \left(\frac{1}{10}\right)^m + \left(\frac{1}{10}\right)^{2m} + \left(\frac{1}{10}\right)^{3m} + \&c.\right\}$$

$$= \frac{R}{10^n - 10^{n-m}} \cdot \text{(cor. prop. 1)}.$$

Where

S represents a given multiple circulate,
n the distance of the *last* figure of the repetend from unity,
m the number of figures that repeat,
R the common numerator.

It is plain from inspection, that m cannot exceed n, but it may be equal or less; therefore, when m is equal to n, then will $S = \frac{R}{10^n - 1}$.

Problem 3.—To find the finite value of any *mixed* circulate.

$$0\cdot3\dot{4} = \frac{3}{10} + S = \frac{3}{10^{2-1}} + S,$$

$$0\cdot30\dot{4}\dot{8} = \frac{30}{100} + S = \frac{30}{10^{4-2}} + S,$$

$$0\cdot046\dot{7}\dot{2} = \frac{46}{1000} + S = \frac{46}{10^{5-2}} + S,$$

$$46\cdot\dot{3} = \frac{46}{1} + S = \frac{46}{10^{1-1}} + S,$$

$$238\cdot00\dot{4} = \frac{23800}{100} + S = \frac{23800}{100^{3-1}} + S,$$

$$374\cdot23\dot{5}\dot{8} = \frac{37423}{100} + S = \frac{37423}{10^{4-2}} + S.$$

Generally, $A = \frac{N}{10^{n-m}} + S$. Where A represents any mixed circulate, and N its finite part. By Problem 2, $S = \frac{R}{10^n - 10^{n-m}}$ therefore $A = \frac{N}{10^{n-m}} + \frac{R}{10^n - 10^{n-m}} = \frac{N(10^m - 1) + R}{10_n - 10^{n-m}}$ .... (a)

Since circulates that begin in the integral part may be reduced to pure ones by dividing by 10, 100, 1000, &c. according to the situation of the decimal point, the quotients thus produced may be found by equation (a), which being multiplied by 10, 100, 1000, &c. (the multipliers agreeing with the divisers) will give the finite value of the mixed circulate proposed. See the following examples:

EXAMPLES.		DATA FOR EQUATION (*a*.)					
No.	Given.	Divide and multiply by	N.	R.	m.	n.	Finite Values.
1	$\cdot\dot{3}$	..	0	3	1	1	$\frac{3}{9}$
2	$\cdot\dot{5}\dot{8}$	..	0	58	2	2	$\frac{58}{99}$
3	$\cdot 000\dot{4}\dot{6}$	..	0	46	2	5	$\frac{\cdot 046}{99}$
4	$\cdot\dot{0}\dot{4}$	..	0	4	2	2	$\frac{4}{99}$
5	$\cdot\dot{9}$	..	0	9	1	1	$\frac{9}{9}$
6	$\cdot 3\dot{4}$	..	3	4	1	2	$\frac{3\cdot 1}{9}$
7	$\cdot 30\dot{4}\dot{8}$	..	30	48	2	4	$\frac{30\cdot 18}{99}$
8	$\cdot 046\dot{7}\dot{2}$	..	46	72	2	5	$\frac{4\cdot 626}{99}$
9	$46\cdot\dot{3}$	..	46	3	1	1	$\frac{417}{9}$
10	$238\cdot 00\dot{4}$	..	23800	4	1	3	$\frac{2142\cdot 04}{9}$
11	$374\cdot 23\dot{5}\dot{8}$	..	37423	58	2	4	$\frac{37049\cdot 35}{99}$
12	$\dot{3}\cdot\dot{4}$	10	0	34	2	2	$\frac{340}{99}$
13	$\dot{3}\dot{4}$	100	0	34	2	2	$\frac{3400}{99}$
14	$2\dot{3}5\cdot\dot{7}$	100	2	357	3	3	$\frac{235500}{999}$
15	$61\dot{4}\cdot 5\dot{6}$	10	61	456	3	3	$\frac{613950}{999}$
16	$7\dot{0}3\cdot\dot{9}$	100	7	39	3	3	$\frac{703200}{999}$

The number of nines in the denominators are equal to the units in *m*.

Article V.

A Reply to Dr. Hope's "Remarks on Mr. Phillips's Analysis of the Pharmacopœia Collegii Regii Medicorum Edinburgensis.[*]

By R. Phillips, FRSE. &c.

Sir,

In the latter part of your remarks upon my analysis, you "earnestly beg" that I will "take the trouble of repeating those processes" of your Pharmacopœia to which I objected. I have complied with your wish in this respect, and if I mistake not, the results are not such as you anticipated. I acknowledge now generally, and shall as I proceed more particularly, state some errors which I committed in my analysis; but I confess I am not a little surprised that you should complain of the tone of my observations. Of yourself I spoke with the respect which I felt, but I trust that I never shall shrink from expressing my opinion of a public performance, on account of any respect for the individuals who may have had a share in its production. I might have stated my objections in more words, but I prefer brevity; and as I am not aware of the cause of your complaint, I shall pursue the same plan in my reply, as I did in what you term my attack.

The first directions to which I shall refer are those for preparing the *acidum aceticum forte*. To these I objected, "that the quantities of the salts employed are not such as are required for mutual decomposition." In your remarks, you say, "the object of this process is to obtain a very strong acid capable of dissolving camphor at a cheaper rate than from acetate of copper."

Before I make any other observation, I must confess that the acetic acid which I have obtained by frequently repeating your process is very much stronger than that which I at first procured; the extreme slowness of the distillation leading me to conclude that it was nearly or quite over, before I had obtained the whole of the acid.

The process for preparing acetic acid, it will be proper to state, for the information of the reader; it consists in decomposing 10 ounces of acetate of lead by 12 ounces of sulphate of iron dried to whiteness. In order to determine the comparative cost of preparing the acid by this process, and by that of the decomposition of acetate of copper, which you consider as more expensive, it will be requisite to state the composition and value of the different substances employed in each method.

According to Dr. Wollaston's scale, crystallized sulphate of iron consists of

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[*] See *Annals of Philosophy* for January and March last.

1 atom of sulphuric acid	50·0
1 atom of oxide of iron	44·5
7 atoms of water	79·3
Giving, as the number representing it,	173·8

When this quantity of sulphate of iron is heated until it becomes white, 69 parts of water are dissipated. It is, therefore, evident (making a small allowance for the error of experiment), that dried sulphate of iron consists of

1 atom of sulphate of iron	94·5
1 atom of water	11·32
	105·82

As then 105·82 of the dry salt require 173·8 of the crystallized, 12 ounces will require 19·7 ounces, the cost of which, as supplied by the most respectable chemical manufacturers, will rather exceed 10*d*.

Instead of purchasing acetate of lead of those who make it for the purposes of the arts, you have directed it to be prepared by dissolving carbonate of lead in vinegar which has been distilled in glass vessels. In calculating the cost of the acetate of lead thus procured, I shall suppose that none of the acetic acid which vinegar contains is lost during distillation, and that it is all converted without loss, and without expense of time, vessels, or fuel, into acetate of lead.

Acetate of lead is composed very nearly of

1 atom of acetic acid	63·96
1 atom of oxide of lead	139·50
3 atoms of water	33·96
Giving	237·42

as the number representing this salt in the scale.

That this is nearly the composition of acetate of lead, may be seen by referring to the analysis of Berzelius; and it is confirmed by the proportion of sulphate of zinc which you have directed for the decomposition of acetate of lead, viz. 60 parts of the sulphate to 80 parts of the acetate, instead of 79, as will be indicated by the scale, provided I have mentioned the correct number for acetate of lead.

As then 237·42 parts of acetate of lead contain nearly 64 of acetic acid, 10 ounces must contain very nearly 2·7 ounces of real acid. One hundred parts of vinegar contain 5 of acetic acid; to procure 2·7 ounces of acid will, therefore, require 54 ounces of vinegar; the cost of which, with the requisite quantity of carbonate of lead, will be about 15*d*.

I have repeatedly prepared your acidum aceticum forte, and the greatest product which I have obtained (quantity and strength being both reckoned) weighed three ounces and a half from 12 ounces of the dried sulphate of iron and 10 of the acetate of lead. The acetic acid contained about 46 per cent. of real acid; the whole product, therefore, contained 1·61 ounce of real acetic acid, at the cost of 2*s.* 1*d.* viz. 10*d.* for the sulphate of iron, and 15*d.* for the acetate of lead. It is, therefore, evident, that by your process the cost of an ounce of real acetic acid is 15½*d.* without any charge for time, fuel, waste, or vessels, either in distilling the acetic acid, or preparing the acetate of lead.

Acetate of copper appears by my experiments, detailed in the last number of the *Annals*, to be a binacetate* of copper composed of

2 atoms of acetic acid	127·92
1 atom of peroxide of copper	100·0
3 atoms of water	33·96

Giving . . . . 261·88 as the number representing it in the scale.

I put into a retort four ounces of crystallized acetate of copper, with two ounces of sulphuric acid and two ounces of water. By distillation I obtained four ounces of acetic acid, containing 50 per cent. of real acid: the cost of the acetate of copper and sulphuric acid amounted to 1*s.* 8*d.*; consequently the cost of one ounce of real acetic acid by this process is 10*d.* The quality of this acid was excellent; and instead of being dearer, as you suppose, than that obtained by your process, it is evidently cheaper in the proportion of 2 to 3.

Having now disposed of the consideration of the "cheaper rate" at which the acetic acid is obtained by the process which you adopt and defend, there are some other points of it to which I wish to direct your attention.

If acetate of lead be the salt selected for decomposition in the retort to procure acetic acid, I must admit that it is better to employ, as you have done, a sulphate containing water in a combined and solid state, rather than by adding sulphuric acid; but it appears to me that you have committed a fundamental error in using sulphate of iron—a salt which consists of one atom of acid and one of oxide, instead of a bisalt, such as bisulphate of copper

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* Since I published this analysis, I have found that Dr. Ure had previously stated the result of his examination in his Dictionary of Chemisty. He makes acetate of copper to consist of

2 atoms of acetic acid	13·26
1 atom of peroxide of copper	10·00
2 atoms of water	2·25
	25·51

We differ as to weight of an atom of acetic acid, and as to the number of atoms of water.

or of potash. When acetate of lead is decomposed by sulphate of iron, two neutral compounds are produced; and it is only from the partial decomposition of the acetate of iron by heat that you procure even the small product of acetic acid which you obtain. Thus 10 ounces of acetate of lead contain very nearly 2·7 of acetic acid, but by your process only 1·6 is obtained.

In my remarks I stated that the quantities of the salts are not such as are required for mutual decomposition; and I mentioned it as probable that the quantity of sulphate of iron was too great in the ratio of 18 to 7·5. By direct experiment, I find, however, that the quantity is still more excessive than I had imagined. Ten parts of acetate of lead require 2·1 of real sulphuric acid for their decomposition; but the 12 parts of dried sulphate of iron which you employ contain 5·6 parts.

But you tell me that it is "of advantage to employ this excess. It facilitates," you say, "greatly the disengagement of the acetic acid, and renders it unnecessary to raise the temperature to so high a pitch as would otherwise be required, by which means the empyreuma, unavoidable in an elevated temperature, is in a great measure prevented."

First, as to the facility of the disengagement of the acetic acid, and the low temperature at which it may be distilled. The term low temperature is of course comparative; but having frequently distilled stronger acetic acid than that obtained by your process by means of a salt-water bath, I put into a retort according to your formula six ounces of dried sulphate of iron and five of acetate of lead. The temperature of the bath is about 224°; and the quantities of ingredients which I used are capable, according to what I have just stated, of yielding one ounce and three quarters of acetic acid. After having continued the heat for five hours, I procured half the quantity of acid obtainable from the acetate of lead; it was, however, disagreeable and empyreumatic. I repeated this experiment, continued the heat for eight hours instead of five, and then obtained exactly 4-7ths of the product yielded by the sand heat. From these statements, I think but little must be said in future about the facilities afforded by the excess of sulphuric acid in disengaging the acetic. Even when the temperature of a sand heat is employed, the operation is excessively tedious, and extremely expensive, on account of the quantity of fuel burned; for in order to obtain the whole product from five ounces of acetate of lead, amounting to only 1 ounce 3-4ths of acetic acid, it required the application of eight hours' fire.

I shall now show that by employing the acetate of lead of commerce, and a much smaller quantity of sulphuric acid than you direct, a much greater proportion of a stronger acid may be procured.

Crystallized bisulphate of potash consists of

2 atoms of sulphuric acid	100·0
1 atom of potash	59·1
1 atom of water	11·32
	170·42

I separately powdered, and then mixed, five ounces of acetate of lead with 3 ounces 5-8ths of bisulphate of potash, containing 2 ounces 1-8th of sulphuric acid, of which one-half, or 1 ounce 1-16th, would act upon and expel acetic acid from the acetate of lead; and it is to be remarked that this exceeds the theoretic quantity requisite, only by about 4 grains. By the heat of a salt-water bath, I obtained in two hours 5-8ths of an ounce of acid, and then removing the retort to the sand heat, there was distilled in less than two hours more, as much acetic acid as made the whole product amount to 2¼ ounces: it contained rather more than 56 per cent. of real acid, or 1¼ ounce, being nearly the whole quantity which existed in the acetate of lead. The acetate of lead for this process costs less than 5*d.*; the bisulphate of potash cannot be valued at more than 2*d.* consequently at an expense of about 5½*d.*, I procure by this process an ounce of real acid, whereas by your method it costs 15½*d.*

Although I consider, for obvious reasons, that the processes which I have described are all preferable to those in your Pharmacopœia, there may be yet some objection made to every one of them. Thus although acetate of copper furnishes a comparatively cheap and really excellent acid, yet it is certainly a costly material. To the use of acetate of lead and bisulphate of potash there are two objections; viz. acetate of lead, always yields an acid which is in some degree empyreumatic, and the oxide of lead forms so insoluble a compound with sulphuric acid, that it is difficult to clean the retort after the operation.

All these difficulties are obviated by a process to which I have already alluded, viz. that of decomposing acetate of lead by sulphate of soda, and then treating the acetate of soda formed with sulphuric acid. The results of this method I shall now state.

Five ounces of acetate of lead were dissolved in water, and decomposed by a solution of 4·3 ounces of sulphate of soda. The solution was evaporated to dryness, the salt reduced to powder, and put into a retort with 1·5 ounce of sulphuric acid and half an ounce of water. In three hours and a half, I procured 2¾ ounces of excellent acetic acid containing 42 per cent. of real acid, or one ounce and one-eighth; so that the ounce of real acid cost rather less than 5*d.*; the sulphate of soda is readily washed out of the retort, and is ready to decompose a fresh portion of acetate of lead. There is generally a small portion of sulphate of lead diffused through the sulphate of soda, but it is easily washed out with it.

Acidum Nitrosum.—In your reply to my objections to this process, you say that I "have thought fit to condemn the proportions directed in the Pharmacopœia as unproductive and injudicious on the result of a solitary trial." It is indeed true that I stated the result of only one trial, but I have made many. I thought it, however, sufficient to relate the results of that one experiment, because it proved that I obtained the whole of the nitric acid capable of being yielded by the nitre within 1-39th part, a loss unavoidable in experiment. I obtained 11·5 parts of nitric acid by using your proportions of 24 parts of nitre and 16 of sulphuric acid. The nitric acid was nearly colourless, and had a specific gravity of 1513; and I endeavoured to show, according to Dr. Wollaston's scale, that the whole quantity of nitric acid procurable from 24 parts of nitre could amount to only 11·8 parts, because the sulphuric acid does not contain water sufficient to condense a greater quantity. To this you reply, that from the use of 24 parts of nitre and 16 of sulphuric acid, the quantity of nitrous acid obtained by you amounts to 15 parts. Now permit me to examine what must occur in this case: 24 parts of nitre consist almost precisely of 12·8 of nitric acid and 11·2 of potash: 16 parts of sulphuric acid are composed of 13·04 of dry acid and 2·96 of water; but I will allow in your favour, that sulphuric acid contains usually one-fifth of its weight of water; we have then 16 parts of it composed of 12·8 dry acid and 3·2 water. Supposing then you condense the whole of the nitric acid, it could amount only to 12·75 parts; and as nitric acid cannot be procured stronger than two atoms of water to one of acid, the nitrous acid which you procure must be composed of 9·55 nitric acid + 3·2 water = 12·75 nitric acid, specific gravity 15, and the remainder of the 15 parts = 2·25 must have been deutoxide of azote condensed by, and converting of, a portion of the nitric into nitrous acid. If then I heat 15 parts of red nitrous acid (so, for distinction's sake, called, but not correctly), I must expel 2·25 to procure pale nitric acid. In order to try this, I put 150 parts of nitrous acid, specific gravity 1522, into a retort to which a receiver was adapted, by the application of heat 27 parts of nitrous acid were distilled, and 114 of pale nitric acid, sp. gr. 1495, were left in the retort; the loss was consequently nine parts. As then 123 parts of red nitrous acid had lost nine parts of deutoxide of azote, 27 the nitrous acid distilled, would lose nearly two more.

We may then, I think, fairly conclude, that 15 parts of red nitrous acid would have lost 1·2 of deutoxide of azote, and that there would have remained 13·8 of pale nitric acid, specific gravity 1495.

The small quantity of nitrous acid distilled had a sp. gr. of 1598, and it would lose, therefore, rather a greater proportion of deutoxide of azote than that of 1522; but, making every allowance, I think it improbable that the nitrous which you obtained should lose so large a proportion of deutoxide of azote as 2·25 parts out of 15, instead of only about 1·2, as by my experiment; and I conclude that the 15 parts of nitrous acid which you obtained consisted very nearly of

Nitric acid, sp. 1495	13·8
Deutoxide of azote	1·2
	15·0

Instead of

Nitric acid, sp. 1500	12·75
Deutoxide of azote	2·25
	15·00

Which must have been its composition, had you procured it from 24 parts of nitre, by the action of 16 parts of the strongest sulphuric acid.

You will perceive that, making an allowance in your favour, the sulphuric acid contained only 3·2 of water, and it could not condense more than 9·55 of nitric acid. I conclude, therefore, either that the sulphuric acid which you employed could not be of the strength directed in your Pharmacopœia, or that you have made some error in your statement.

It is evident, from what I have just stated, that I have been able to procure nitrous acid of specific gravity 1522; and here I must acknowledge that I was not aware that nitrous acid could be obtained of a "full red" colour, without employing impure nitre, or without using some means to convey the gaseous products arising from the decomposition of nitric acid through the acid distilled. I repeat that I was not aware that by mere exposure to the gaseous products, the nitric acid first distilled would absorb sufficient deutoxide of azote, to be in any considerable degree converted into red nitrous acid. By continuing the application of heat, however, for a great length of time, I procured red nitrous acid, of specific gravity 1534; but the quantity which I obtained convinced me that, as I have already endeavoured to show, your statement must be incorrect, or that you employed sulphuric acid differing in strength from what is directed. Twelve ounces of nitrate of potash and eight ounces of sulphuric acid, of specific gravity 18·475, were put into a retort, and heat applied until acid ceased to distil. The product was of specific gravity 1534, it weighed six ounces six dr.; showing that 24 parts would yield only 13·6 parts instead of 15, when acted upon by 16 parts of sulphuric acid, and of these 13·6, the water must have constituted 3·2 parts; nitric acid, 9·5, and the remaining 0·9 part will be accounted for by the loss of deutoxide of azote, as already noticed.

You assign as reasons for employing the proportions of 16 parts of sulphuric acid to 24 of nitre, first, "that the prescribed quantity of sulphuric acid is required, and proves sufficient to detach the whole of the acid from the nitre; secondly, the acid thus procured is of great strength, and so free from sulphuric acid, as to render the second distillation enjoined by the London College altogether unnecessary for ordinary purposes." Now in reply to these statements, I would observe, first, that in my opinion, and founded upon experiment, the quantity of sulphuric acid is not required to detach the whole of the acid from the nitre; and, secondly, that although it does detach it, yet only three-fourths are obtained when the sulphuric acid is of the greatest strength, on account of the deficiency of water in it to condense the nitric acid.

I mixed in a retort 100 parts of nitre and 50·6 parts of sulphuric acid, of specific gravity 18435, which are equivalent to about 48·87 of sulphuric acid of the greatest procurable density; the sulphate of potash obtained weighed 86·2 parts, exceeding only by 0·2 the quantity mentioned on the scale. By passing the gas liberated towards the end of the operation through water, I obtained nitric acid equivalent to 46·2 solvent power instead of 50, as denoted by the scale. From this experiment I contend that it is not requisite to employ more sulphuric acid than is required to convert the nitrate into sulphate of potash. It is indeed true that about 1-12th of the product is lost in the operation. Now I will even grant for a moment that you obtain 15 parts of nitric acid of the greatest density and solvent power from 24 parts of nitre, instead of 15 of red nitrous acid, as you state; these 24 parts of nitre are capable of yielding 17 of such acid, and it is, therefore, evident that, although you decompose the whole of the nitre, from the want of water to condense the product you lose more than one-eighth of it.

In one part of your remarks, you say, "Permit me to observe that most of your objections to the formulas apply to the relative quantities of the materials employed, and rest upon these quantities deviating from the proportions of combination stated in Dr. Wollaston's table of chemical equivalents. I apprehend, however, that you have made an application of this beautiful and valuable contrivance which its very ingenious author never contemplated, and could not now sanction; for though that table displays the proportions in which different substances combine, it by no means displays the relative quantities of the substances to be employed when decompositions are to be effected, particularly by single affinity." I think you must have overlooked a part of Dr. Wollaston's memoir which refers to the very subject under discussion, and in which the number that he employs in describing the relative quantities of sulphuric acid and nitre to be used are taken from the scale which he had been describing. "In the distillation of nitric acid from nitre," says Dr. Wollaston, "the whole of the acid may be obtained, if we employ enough of sulphuric acid to convert the residuum into bisulphate of potash. In this case each portion of potash, from which dry nitric acid is separated, will displace the water from the two equivalent quantities of sulphuric acid, and each portion of nitric acid weighing 67·54 will be found combined with 22·64 of water. Hence 90·18 of liquid nitric acid so obtained should dissolve the equivalent 63 of carbonate of lime;" and the author then goes on to show from some experiments which I had made, that this is as nearly as possible actually the case, and that the specific gravity of the acid was 1·50.

I conceive it impossible to cite a more direct application of the scale to the purpose for which I had ventured to employ it. We have not only the quantity of the "decomposing material" assigned, but its composition stated, and also that of the substance to be decomposed, and of the product and residuum. With respect to the strength of the product, it is quite as great whether we use only 12 parts of acid or 24, instead of 16 as you direct. I do not speak from theory, but from examining the nature of the nitric acid produced with all three proportions of sulphuric acid. As to its being so free from sulphuric acid as not to require the second distillation enjoined by the London College, I beg to state that the redistillation ordered by the London College is useless; for I never found the product to contain any sulphuric acid.

Acidum Nitricum.—With regard to this article, you at once state that my criticism of it appears to you "to be incorrect in every point." And you assert, "that though, for many purposes, the nitric and nitrous acids may be used indiscriminately, yet there are some where they cannot with propriety." I wish you had pointed out one of those cases. I will not deny that such exist, but I know of no one. Allow me to inquire whether there is any case in which the nitrous acid is not either previously to, or during its use, converted into nitric acid? But allowing this for a moment not to be the fact, is there any instance in which it is preferable to use nitrous than nitric acid? I shall presently show, that unless pressure be used so as to condence that part of the nitric acid, which is liberated in the state of gas when only one atom of sulphuric acid is used to decompose nitre; that it is more advantageous in every respect to use equal weights of sulphuric acid and nitre than two parts of one and three of the other, as ordered in your Pharmacopœia.

You tell me that I "have assigned as a reason for preferring the process of the London College, should a pale acid be wanted, that it affords it by one operation; while that of the Edinburgh requires two, but I had forgotten you state that the London Pharmacopœia directs a second distillation of a fresh quantity of nitre, and after all does not procure a colourless acid." You have, I am sure, unintentionally misrepresented me on this occasion. What I state is, "it would be better to adopt the London process of using equal weights of nitre and sulphuric acid," but this is the only part of it which I meant to adopt. Indeed it would be singular if I had done more, for in my Experimental Examination, I have stated that I never found any sulphuric acid in the product. It is also true that the nitric acid obtained by using the proportions of the London Pharmacopœia is not quite colourless, but it is very nearly so: scarcely any difference can be perceived between it and that procured by your method, and it is quite as strong, or indeed rather stronger. With respect to the colour of the acid, you correctly inform me that when nitrous acid is diluted with water, it becomes of a rich green colour; but I can by no means admit the accuracy of your intimation, that it requires "free exposure to the air" to render it pale; for without it is kept quite closely stopped; and in a bottle but little larger than requisite to hold it, the very act of opening the bottle occasionally is sufficient to admit of the escape of the deutoxide of azote, and to render the acid colourless.

As you have spoken of the economy of the process for obtaining acetic acid, it is evident that this is a consideration which has not escaped your notice, although I have shown that you have not been successful in the practice of it. I shall now briefly state the comparative costs of obtaining nitric acid by employing one proportion and two proportions of sulphuric acid; and also by your method in which one proportion and a half is used. The numbers representing sulphuric acid and nitre are respectively 61·32 and 126·6. For the sake of round numbers, I shall assume that they are as 1 to 2.

I distilled a mixture of 24 parts of nitre and 16 of sulphuric acid. In four hours, from the commencement of the operation, the ingredients were in complete fusion, and no uncondensible gas came over. In about five hours, a little gas was evolved, the acid dropping only about three times in a minute. In two hours more, I weighed the product, and found it almost exactly 11 parts; in eight hours from the commencement of the operation gas came over plentifully, and the acid dropped once in about five minutes. The whole product weighed 13·4 parts. It was of a red colour, but its specific gravity I accidentally have not noted. The pipe of the tubulated receiver almost touched the bottom of the bottle, so that any gas which was evolved in the latter part of the operation passed through the pale fluid acid first distilled.

On repeating this experiment, but without causing the pipe of the tubulated receiver to dip into the first obtained product, I procured 13·6 parts of product. This acid was of a red colour; its specific gravity was 1534; and on making the experiment for the third time, but with the pipe of the tubulated receiver arranged as in the first experiment, I procured 13·7 parts of acid; the colour was red, and its specific gravity 1540.

I now distilled a mixture of 24 parts of nitre and 24 of sulphuric acid. The product was of a pale colour, of specific gravity 1499, and weighed 16 parts within a few grains. On comparing it with the nitric acid procured in your mode, there was scarcely a shade of difference in their colour. The whole quantity of nitric acid, of specific gravity 1500, obtainable from 24 parts of nitre, amounts to 17 parts; so that a little more than 1-17th was lost in the operation.

Now the cost of 24 parts of nitre and 24 of sulphuric acid being estimated at 144, that of 24 parts of nitre and 16 of sulphuric acid will be 128, and the acid produced by the former amounts to 16 parts, and that by the latter to 13·7 parts. It follows, therefore, that 16 parts of pale acid obtained by using 24 parts of sulphuric acid cost less than as much red acid obtained by using 16 of sulphuric acid, in the proportion of 144 to 149; for 13·7 : 128 :: 16 : 149. This calculation is made on the supposition that red nitrous acid, of specific gravity 1534, is equal in strength to pale acid of 1499, which, however, is far from being the case; for, as already shown, both from theory and experiment, the 3·2 parts of water which 16 of sulphuric acid contain can yield only 12·7 of pale acid; therefore, the cost of your method is greater than that incurred by using one half more sulphuric acid in the proportion of 161 to 144; for 12·7 : 128 :: 16 : 161.

You do not seem to suppose that nitrous acid is applicable to any one medicinal purpose which may not be as well answered by using nitric acid; but unless you are prepared to show this, I think I have proved that your process is for several reasons much less advantageous than that which Dr. Wollaston has described, and I have quoted from him. Your process, from the deficiency of water to condense the nitric acid, is less economical; and you incur some expense of fuel and increased risk of breaking the retort, merely to render the acid red; and when this is done, it is to be rendered colourless by incurring fresh expense of time, fuel, and the chance also of breaking the vessel in which the operation is performed.

Acidum Muriaticum.—With respect to this preparation I have no hesitation in stating that I was in error in supposing that the quantity of sulphuric acid which you employ is less economical than that indicated by the scale as equivalent to the common salt. The experiments upon which my present opinion is founded are the following:

According to your process, I mixed in a retort 10 parts of sulphuric acid with 10 of common salt, and the requisite quantity of water. The cost of the salt being estimated at 4, that of the sulphuric acid will be 2, total cost = 6. When the operation was over, I tried the specific gravity of the muriatic acid, and found it 1176, and the residuum weighed 13·45 parts. We may, therefore, presume, that the whole of the common salt was decomposed; for 10 parts of sulphuric acid contain 8·15 of dry acid, and 10 parts of salt yield 5·35 soda; the weight of the residuum should, therefore, have amounted to 13·5 parts.

Operating in the same manner, I used, according to the scale, the equivalent quantities of sulphuric acid and common salt; viz. 8·4 of the former, and 10 of the latter, the cost being 1·68 for the sulphuric acid, and 4 for the salt, amounting to 5·68. On examining the residuum, I found that 1·7 part of the salt remained undecomposed, showing that the expense of decomposing 8·3 parts of it amounted to 5·68; whereas by your process it is rather less than 5; for 10 : 6 :: 8·3 : 4·98.

While, however, I allow the preference that ought to be given to your process, I cannot admit the justness of the reasoning upon which you found its superiority. You observe that it must "for the moment have escaped my recollection, that sulphuric acid is much disposed to form a supersulphate of soda, and consequently that if no more acid be employed than is barely sufficient to saturate the quantity of soda contained in the muriate, a considerable portion of the muriate will remain undecomposed." Now if the decomposition of the common salt depended upon the formation of bisulphate of soda, as you seem here to hint, you ought to have employed 16·8 of sulphuric acid, instead of only an equal weight, with 10 of common salt. It is, however, quite evident, that the decomposition of the common salt is not dependent upon the formation of a bisulphate; for not only, as I have just shown by your process, 10 parts of sulphuric acid are capable of effecting what ought to require 16·8; but in the other experiment which I have detailed, 8·4 parts of the acid decomposed 8·3 of common salt, which are the quantities requisite to form common sulphate of soda as nearly as 8·4 to 10.

Aqua Potassæ.—From repeated experiments on the subject, I am convinced that the quantity of lime which you order is unnecessarily large; and that even supposing you obtain, as you assert, the whole of the potash, the method is tedious on account of the long time which it occupies.

Subcarbonas Ammoniæ.—On this head also I must retain my former opinion; for where the equivalent quantities are 100 of muriate of ammonia to 95 of carbonate of lime, I conceive it to be utterly impossible that it can be of any use to employ twice the quantity of the carbonate, as you direct, although I am perfectly willing to admit, as with respect to the preparation of muriatic acid, that an excess of the cheaper material ought always to be employed. You will find in my examination of the London Pharmacopœia, in treating of the preparation next to be noticed, that I propose to use a considerably larger quantity of lime than the equivalent.

Aqua Ammoniæ.—The process of the Edinburgh Pharmacopœia consists in decomposing 12 parts of muriate of ammonia by means of 18 of lime previously slaked with nine parts of water, the gas evolved being condensed by 12 parts of water. To this process I objected that as muriate of ammonia is decomposable by little more than half its weight of lime, that one and a half its weight is uselessly and inconveniently large, I objected also that the operation of mixing lime and muriate of ammonia was extremely pungent, and that the large quantity of lime delayed the operation of getting the ingredients into the retort. I objected also, that as the retort was ordered to be made red-hot, that it usually broke in the operation. In this I find I committed an error, and I am afraid that it arose from confounding your process with that of the London Pharmacopœia of 1809. I must, however, confess that I cannot find much real difference in the directions. If iron be made red hot, I think it would be difficult to determine that the sand placed upon it, and the retort also placed in the sand, are far from possessing an equal temperature; but you tell me, that "as much of the heat is carried off by the gas, the temperature of the mixture is in every stage very far distant from the point of incandescence." By the heat being carried off by the gas, I conclude that you mean it is rendered latent, but for two reasons I do not see how this can happen: the first is, that without the application of any heat whatever, so much ammoniacal gas is evolved as to create the pungency of which I complain from the mere mixture of the lime and muriate of ammonia; and secondly, the ammonia during condensation scarcely raises the temperature of the water. If then ammoniacal gas may be evolved without the application of heat, I do not see how it can carry any off when given out during its operation; and I think that during condensation such heat would be again given out; but this, as I have just stated, does not appear to be the case.

In order to determine whether any greater product is obtainable by using so large a proportion of lime as you order, I twice prepared the solution according to your directions. The mean specific gravity of the aqua ammoniæ obtained was 0·936, being rather stronger than you state: the mean quantity of ammonia estimated by Sir H. Davy's table of the strength of ammoniacal solutions amounted to 40·5. I then prepared aqua ammoniæ, using only two-thirds of the quantity of lime ordered in your Pharmacopœia, and this quantity, I think, needlessly large. Upon comparing the mean strength of the products with those obtained by the Edinburgh process, I found it to be exactly 40, the difference being only 0·5, which may be fairly attributed to the error of operating; for I found greater differences than this between the values of the two products in both cases. Now even this diminution of the quantity of lime from three parts to two is important, because, when slaked, it is extremely bulky, and therefore, requires large retorts, which increase the expense, especially as, according to my experience, the retorts break twice out of three times, and the value of them is at least eight times greater than that of the substances operated upon; it is on this account that

I prefer the process of the London College; for although, as I have before acknowledged, it is in point of strength of less value than yours in the proportion of 10 to 16; yet there is so much less risk in heating a fluid in a retort than solids that I would much rather engage to prepare a given quantity of ammonia in solution by the London than by the Edinburgh process; added to which, the mixing of the ingredients in the manner directed in the latter is an extremely unpleasant operation, and totally unfit for the preparation of large quantities of the solution.

Tartras Antimonii.—In reply to my remarks on this preparation, you say, "the name given to this substance naturally first excites your animadversion. Had you happened to look at the preface to the Pharmacopœia, you would have found the reasons assigned by the College for deviating occasionally from that nomenclature, and abbreviating the name of some compound substances, for the sake of convenience in prescription, by restricting it to that of the active ingredient. The tartras antimonii is one example." Now I will admit that it is highly advantageous to shorten the name of preparations; but in doing this, it would, I think, be better to give an arbitrary name than one which conveys an incorrect idea of the nature of the substance. *Antimonium tartarizatum*, for example, is an appellation which conveys no false idea of the nature of emetic tartar; but, I think, as potash is one of its ingredients, that tartrate of antimony does give an incorrect representation of this substance. I will, however, admit for a moment, that tartrate of antimony is a proper appellation; why then is not tartarized iron subjected to the same rule? This compound is correctly called *tartras potassæ et ferri*, which is only four letters shorter than the name of *tartras potassæ et antimonii*. But there is another preparation of this same metal to which your rule would have been more applicable than to emetic tartar;—I mean *oxidum antimonii cum phosphate calcis*. If the name of potash may be omitted from *tartras potassæ et antimonii* because it is not the "active ingredient," surely the same law might, *a fortiori*, have been applied to the *oxidum antimonii cum phosphate calcis*: omitting the name of the bone-earth, and calling it *oxidum antimonii* would have been consistently "restricting it to that of the active ingredient."

With respect to the different methods of preparing tartarised antimony, you inform me, that with the aid of Dr. Duncan, jun. Professor of the Institutes of Medicine, all the processes lately recommended by the Colleges of London and Dublin, and by me, were carefully tried; and you saw no reason for preferring any of them to the one in your former editions. I confess I wish you had gone rather further in your remarks, and had shown the grounds upon which you prefer the process to which I have objected. As, however, you have not done this, I shall venture to mention my reasons for thinking that the process which I recommended in my examination of the London Pharmacopœia is preferable to that which you have adopted from the former editions of your Pharmacopœia. Your process consists in deflagrating a mixture of equal weights of nitre and sulphuret of antimony, the white crust formed at the surface being separated from the red under part; the latter is to be reduced to a very fine powder, frequently washed with warm water, dried, and then boiled in water with an equal weight of bitartrate of potash.

The objection which I made to this process is, that unless the sulphuretted oxide, formerly called *crocus antimonii*, thus prepared, be made to undergo the troublesome process of levigation or elutriation, it is very difficult to cause the tartar to dissolve a sufficient quantity of it. I prepared some of this sulphuretted oxide in the manner directed, and having reduced it to an extremely fine powder, and repeatedly washed it, I boiled 100 parts of it in water with an equal weight of tartar during a much longer time than you direct, in order to insure their mutual action to the fullest extent. Having filtered the solution, I found that only 60 parts of the crocus were dissolved, although when it has been levigated, the tartar is capable of dissolving 75 instead of 60; consequently one-fifth of the tartar was uncombined with oxide, and must yield a product intermixed with bitartrate of potash. From repeated experiments, I am also of opinion that a larger quantity of the crocus should be used than you direct; at least one-tenth more even when levigated.

The process which I proposed in the examination of the London Pharmacopœia consisted in boiling antimony with sulphuric acid so as to convert it into subsulphate, to wash this, and then dissolve it in a solution of tartar. Now I prefer this process, because I find that it occupies scarcely two-thirds of the time required by yours. A very slight degree of washing is requisite; the subsulphate of antimony is dissolved in a few minutes, instead of requiring an hour's boiling, as you direct; the tartar being even then imperfectly saturated: added to this, it is not requisite to dry the antimonial subsulphate, as you direct, with the sulphuretted oxide. With respect to cost, I am at present uncertain; because I have not determined the exact quantity of sulphuretted oxide yielded by certain proportions of nitre and sulphuret of antimony; but if you obtain nearly the whole of the antimony, then, at the present price of nitre, your method is more economical as far as the first cost of materials is concerned; but the use of glass vessels, the long boiling, and the unnecessary drying of the oxide, all tend materially to diminish this advantage of the cheapness of the first cost of the ingredients.

Acetas Hydrargyri.—For preparing this compound, you direct three parts of mercury to be dissolved in four ounces and a half of dilute nitrous acid, or a little more than is required; and this solution is to be decomposed by one of acetate of potash.

In preparing the *submurias hydrargyri præcipitatus*, you order the mercury to be dissolved by an equal weight of dilute nitrous acid. Now observing this difference, and well knowing that the preparation of the acetate and the chloride of mercury depend upon the formation of protoxide, I certainly did imagine that the same quantities of metal and acid were applicable to both cases. You tell me, however, that the College are not inconsistent in ordering an excess of nitric acid in one case, and not in the other: for you state that the excess is "advantageous for the one, and prejudicial for the other."

Now the advantage which you state to be derived from using the excess of nitric acid in preparing the acetate is, that it dissolves the subnitrate, which would otherwise be precipitated with, and contaminate the product. For a moment I will admit this reason to be valid; but if it be so, what prevents the same effects from being produced when a solution of muriate of soda is used instead of acetate? I apprehend it is the addition of water, and not the nature of the salt dissolved in it, which determines the precipitation of subnitrate of mercury; if so, it appears to me that you are inconsistent in not ordering the excess of acid in preparing the chloride, as well as in forming the acetate of mercury.

You will probably remember, that in analyzing calomel prepared by precipitation, Mr. Chenevix actually found a quantity of subnitrate; I do not, however, mean to assert that the proportions of mercury and acid which he used in preparing it were such as are directed by you; I mean merely to show that this effect in some cases actually happens.

I will again admit that the excess of acid is requisite to produce the alleged effect; but even in this case, I think it is employed in a disadvantageous mode. I suppose you will agree with me, that it is only the peroxide of mercury which is subject to be precipitated in the state of subnitrate; if so, the formation of this oxide must be much increased by using one-half more acid than is requisite for the preparation of the protoxide. It seems to me that this end would be quite as well answered, and with much less chance of producing peroxide of mercury, if the solution prepared with only the requisite quantity of acid were afterwards diluted with water, with which the excess of acid had been mixed; in this mode, as it appears to me, it would retain its power of redissolving subnitrate, without possessing that of forming peroxide.

In my analysis of your Pharmacopœia, I stated that I procured only 29 of acetate of mercury from a solution of 72 of the metal. I have since obtained a considerably larger product; viz. 36 parts; still, however, the loss of mercury is extremely great. Upon a moderate calculation at least 44 parts of 72 of mercury are unemployed in the formation of acetate; and this proportion I certainly am still of opinion that it would be advisable to save first by the addition of solution of muriate of soda, so as to obtain calomel, and then by adding potash to procure peroxide.

Wishing, however, to determine, whether when mercury is dissolved without heat in an equal weight of your dilute nitrous acid, any subnitrate of mercury is actually precipitated with the acetate, I added to a solution thus prepared the requisite quantity of acetate of soda dissolved in only half the quantity of water which you direct. The acetate of mercury, instead of weighing only six parts from 12 of mercury, as by your process, amounted to 11 parts; and the only important difference between the products, if indeed it can be called one, was, that by using a smaller quantity of water, the precipitate was rendered less bulky. In order to determine whether it contained any subnitrate, I boiled some of it in muriatic acid; calomel was of course formed and precipitated; to the filtered solution I added ammonia, which did not, however, cause any precipitation; whereas, if the acetate had contained any subnitrate, it would have been decomposed by, and dissolved in the muriatic acid, and the permuriate formed would have given the well-known white precipitate with ammonia. On repeating this experiment I obtained rather a larger product of acetate. From these experiments, therefore, I am of opinion that it is not requisite to use the excess of acid which you have ordered, but that this acid converts a large portion of mercury into peroxide which is of no use in forming the acetate, and that it also diminishes the product, by dissolving the acetate of mercury actually formed. If it be of any consequence that the acetate of mercury should be extremely light, it would be better to redissolve and crystallize it, than occasion, what appears to me to be, waste by your process.

Oxidum Hydrargyri Præcipitatum.—I have already acknowledged the error which I committed with regard to the quantity of lime contained in the lime water, ordered for the decomposition of the calomel in this process. But Mr. Dalton has observed, and I have repeated and proved the accuracy of his experiments, that lime water, prepared in different modes with respect to temperature, contains very different proportions of lime. Under these varying circumstances, I confess I do not consider it to be an eligible substance for the purpose to which it is applied in this process. If the lime be redundant, it will be precipitated by the heat, and mix with the oxide of mercury; if, on the other hand, it be deficient, it will leave some calomel intermixed with the oxide. Added to this, protoxide of mercury, at a very moderate elevation of temperature, readily absorbs oxygen; so that a portion of it passing to the state of peroxide, the precipitate becomes of a greenish colour, and is a mixture of the two oxides.

I find that when potash is used, none of these inconveniences follow. The calomel is decomposed even without the assistance of heat, and a few ounces of solution of potash are as effective as an equal number of pints of lime water. To some of the oxide of mercury obtained by potash, I added nitric acid; it was readily dissolved, and to the solution I put some nitrate of silver. Not the slightest precipitation of chloride of silver occurred, showing that the decomposition was perfect. This operation requires only a few minutes for its completion.

Subsulphas Hydrargyri Flavus.—You have not thought it worth while to notice an inconsistency which I pointed out in your directions for this preparation. It is to be formed by boiling together two parts of mercury and three of sulphuric acid; whereas in preparing corrosive sublimate, the proportions are two of mercury and two and a half of acid. I cannot imagine any cause for the variation, which, however, is not very important.

Murias Hydrargyri Corrosivus.—I have not had time to determine experimentally what proportion of common salt is required for the decomposition of sulphate of mercury; but until I am convinced by experiment, I think I shall retain my opinion, that the quantity which you have directed to be used is very unnecessarily large.

Oxidum Hydrargyri Rubrum per Acidum Nitricum.—Permit me to remark, that this is one of those names which would have been improved, by subjecting it to curtailment equal to that bestowed upon the tartras potassæ et antimonii.

For preparing this oxide of mercury, you direct three parts of mercury to be dissolved in four of dilute nitrous acid, and to these proportions I objected because three parts of acid are equal to produce the effect, as ordered in preparing the submurias hydrargyri præcipitatus. In your reply, you say, "If the diluted nitric acid have been formed from the strongest acid, you are perfectly correct in saying, that it will dissolve an equal weight of mercury, and in that case more acid is ordered by the Edinburgh College than is necessary for preparing this substance. But if the best acid usually met with in apothecaries' shops be employed, in consequence of its inferior strength, the proportion assigned in the Pharmacopœia will be found most suitable."

Now this, I must confess, appears to me to be a most extraordinary kind of defence. You tell me that your Pharmacopœia possesses "a certain degree of national authority in the preparation of drugs;" but I think you would have been more discreet in allowing, that I had detected an error of no very great importance, than thus to admit that the authority which you possess is of so trifling a nature, that you legislate in one part of the Pharmacopœia to accommodate those who contravene the directions of the other. This declaration perfectly satisfies me, that however I might have flattered myself as to the utility of my remarks, nothing which I have yet done, or ever can perform, will be in the least degree advantageous to your Pharmacopœia. Hoping, however, that the public may derive some utility from the experiments which I have detailed, I am, Sir, yours, very respectfully,

RICHARD PHILLIPS.

ARTICLE VI.

Extract of a Memoir on the Influence of the Nervous System on Animal Heat.[*] By Dr. Chossat.

MR. BRODIE, in his important researches on the Influence of the Brain on the Action of the Heart and Animal Heat, has shown, first, that after decapitation the animal heat diminishes several degrees in one hour, notwithstanding the artificial inflation of the lungs; and, secondly, that the animals decapitated and inflated cool more rapidly than those killed merely by the section of the spinal marrow under the occiput; and thus, after decapitation, no perceptible quantity of heat is produced. I propose in this paper to analyze these important results, and inquire in what manner the nervous system can influence the production of animal heat.

As preliminaries, I shall mention some considerations necessary for the better understanding of the facts afterwards to be related; viz.

1. *The Phænomena of Death by Cold.*—In a great number of experiments made with my friend Dr. Prevost, (yet unpublished) we have seen in dogs[†] placed in the cold bath, death take place at 26° centigr., and below this point in proportion to the slowness of the refrigeration of the body. At the autopsy we have found a nearly total extinction of muscular irritability and peristaltic motion, blood, commonly arterial, in the lungs and aorta, and some serum in the ventricle of the brain.

2. *The Progress of Refrigeration after Death.*—I propose to compare this refrigeration with that which happens after the wounds inflicted on the nerves, in order to determine the influence of this system in the production of animal heat.

Exper. I.—In an animal which died from syncope soon after the section of the spinal marrow, the initial temperature being at the moment of death 40·5°, I found that

From 40·0° to reach 31·7 .... 3^{h} 30' were employed.
31·7 23·9 .... 7 0

Dividing the number of degrees by that of the hours employed, we obtain, what I shall call hereafter, the average refrigeration, *i. e.* the average diminution of animal heat in one hour between such limits as we think proper to choose. Adopting the limits of $\left\{\begin{matrix} 40^\circ \text{ to } 32^\circ \\ 32 \text{ to } 24 \end{matrix}\right\}$ we find, according to the preceding date,

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[*] Presented to the Academy of Sciences on May 15, 1820. An extract of this paper has already appeared in the Annales de Chimie et de Physique, from which the present article was translated, with some variations and additions, by the author, during his late residence in London.—*Ed.*

[†] All the experiments related in this paper have been performed on dogs.

$$\text{Average refrigeration, first limits} = \frac{8{\cdot}3^\circ}{3^{h}\ 5'} = 2{\cdot}37^\circ \text{ per horam}$$

$$\text{second limits} = \frac{7{\cdot}8^\circ}{7^{h}} = 1{\cdot}11^\circ \text{ per horam.}$$

After these preliminaries, I enter upon the proper subject of the paper.

To Mr. Brodie's experiments, the following objections might be made: 1. That pulmonary inflation after decapitation is a cause of refrigeration capable alone of killing the animal. 2. That the section of the par vagum, and consequently the decapitation, produces an infiltration of the lungs which alters the chemical processes of respiration. It is necessary, therefore, to examine what effect such wounds of the brain which do not interrupt the respiration would produce, and still leave the lungs under the influence of the par vagum. I succeeded by the following experiment:

Exper. 2.—A vertical section of the brain was made a little before the pons varoli: spontaneous respiration: death the 12th hour, with all the signs of death by cold. The animal heat descended from 40° to 24°, but in a progression a little different from that of the refrigeration after death (Exper. 1), as shows the determination of the average refrigeration between the limits chosen for the first experiment. I find in the second experiment:

$$\text{Average refrigeration} = \frac{40{\cdot}0^\circ - 31{\cdot}7^\circ}{2^{h}\ 30'} = 2{\cdot}93^\circ \text{ per horam.}$$

$$\frac{31{\cdot}7^\circ - 24{\cdot}0^\circ}{9^{h}} = 0{\cdot}85^\circ \text{ per horam.}$$

Exper. 3.—Violent commotion of the brain followed by a complete loss of sensibility, and the cessation of respiration. Artificial inflation of the lungs during the whole experiment. Death the 11th hour at 22·3°.

$$\text{Average refrigeration} = \frac{39{\cdot}3^\circ - 31{\cdot}7^\circ}{3^{h}\ 50'} = 2{\cdot}17^\circ.$$

$$\frac{31{\cdot}7^\circ - 23{\cdot}9^\circ}{6^{h}} = 1{\cdot}30^\circ.$$

Opium acting especially on the brain, its influence on animal heat was tried in the following experiment:

Exper. 4.—Injection in the jugular vein of 0·3 of grain of crude opium boiled in 16 grammes of water. Death the 22d hour at 22·8°.

$$\text{Average refrigeration} = \frac{39{\cdot}8^\circ - 31{\cdot}9^\circ}{3^{h}\ 30'} = 2{\cdot}25^\circ.$$

$$\frac{31{\cdot}9^\circ - 23{\cdot}9^\circ}{15^{h}\ 5'} = 0{\cdot}53^\circ.$$

Comparing the average deduced from the first part of the second, third, and fourth experiments, viz,

Section of the brain	2·93°
Violent commotion	2·17
Opium	2·25
Average.	2·45

We see very different experiments affording nearly the same result. It was then natural to deduce the existence of a general cause independent of the form of the experiments, and which, in these three instances, had acted in an entirely similar manner.

Now the circumstance common to these three experiments being the more or less complete abolition of the functions of the brain, it was natural to enquire if it was not in the organs placed under the immediate influence of the brain, that the real cause of the diminution of animal heat would be found. Two queries to resolve presented themselves: the one, to inquire whether the refrigeration did not depend on the cessation of the influence of the par vagum; the other, whether this same effect did not originate from the paralysis of spinal marrow. I am to detail the results of these two series of experiments, beginning with those which relate to the par vagum.

I. *Section of the Par Vagum.*

Legallois concluded from his experiments, that after the section of the par vagum the animal died from asphyxia, occasioned by the infiltration of blood, or serum, into the substance of the lungs. My own researches, though confirming the truth of this observation in young rabbits (on which Legallois seems to have principally operated) induce me to doubt this assertion as to its generality. So far from dying from asphyxia, I found arterial blood in the substance of the lungs of full grown dogs, and sometimes in the aorta itself. The cause of death in such animals is the progressive diminution of animal heat; for life ceases only when the refrigeration is great enough to produce death necessarily, and independently of every other cause.

Exper. 5.—A tube was introduced into the trachea to prevent the dyspnea consequent to the section of the nerves. The two pneumogastric nerves were cut. Death took place the 60th hour at 20·7° During the 36 hours, immediately consecutive to the operation, numerous oscillations of the animal heat were observed between 36° and 38·6°.

In three sections of the par vagum, the average refrigeration per hour was for the first part of the experiments:

Exper. 5.	0·09°
Exper. 6.	0·25
Exper. 7.	0·45
Average.	0·26

Comparing this average with that of the Experiments 2, 3, and 4 (which we have found to be 2·45°), we see that the animal heat diminished 10 times more rapidly in these last experiments. The influence of the par vagum cannot, therefore, explain the quickness of the refrigeration occasioned by acting upon the brain; which conclusion is also confirmed by the result of the fourth experiment, in which we have seen the animal heat diminish rapidly, though the respiration was performed freely.

For the second part of the experiments, however, we do not find more than this difference in the average refrigeration, as it appears from the following table, which comprehends both the section of the par vagum, and the refrigeration after death:

	Average refrigeration. (Second part of exper.)	
Exper. 1. (Refrigeration after death)	—	1·11°
Exper. 5. (Section of the par vagum)	1·06°	—
Exper. 7. (Section of the par vagum)	1·47	—
Average.	1·26	1·26

I conclude then that after the section of the eighth pair, the production of animal heat is yet taking place, though in a diminished proportion, as long as the temperature of the body remains higher than about 32°, but below this point, the animal cools as if it were dead.

I pass now to the second of the two queries proposed.

II. *Influence of the Spinal Marrow on Animal Heat.*

(*a.*) *Sections of the Cervical Part of the Spinal Marrow.*—All the sections performed on this part of the spinal marrow presented the same results, whether artificial respiration was performed, as in the superior intervertebral spaces; or the respiration was quite free, as in the inferior of these spaces. It will be sufficient, therefore, to present in this extract one only of these experiments.

Exper. 8.—Section performed between the last cervical and the first dorsal vertebra. Death the 10th hour at about 24°.

$$\text{Average refrigeration} = \frac{40{\cdot}0^\circ - 32{\cdot}3^\circ}{2^{\text{h}}\ 45'} = 2{\cdot}80^\circ$$

As it appears from this experiment, we find in the whole length of the cervical part of the spinal marrow the same average diminution of animal heat in the section of the brain. It is then no longer possible to admit that the animal heat is under the immediate dependance of the brain, and it is natural to conclude, that decapitation influences animal heat so remarkably only from the influence exercised by the brain on the functions of the spinal marrow.

(*b.*) *Sections of the Dorsal Portion of the Spinal Marrow.*— Without entering into the particulars of any of these experiments, which will be found in the original paper, I shall relate only, that we see now, for the first time, a phenomenon which we did not observe in any of the former experiments; viz. that from the section in the fourth or fifth intervertebral space of the dorsal part of the spine, the refrigeration of the body is no more continued, but is interrupted by a period of reaction, which is of variable duration (some hours), the true image of a fit of fever, and characterized by shivering, a quick and hard pulse, and the increase of animal heat. This increase is in general the more slow and feeble as the section is made higher up,* and has elevated the animal heat in the lower of these spaces higher than its original standard (in the last of these spaces I have observed the 12th hour from the operation 42·9°, the original state being 41·1°). This complication preventing us from deducing the average refrigeration from these experiments as from the preceding, I have chosen for them another mode of comparison; that of comparing the greatest refrigeration observed in the three first hours consequent to the operation, without paying any attention to the reactions which might happen in the interval. It is on this principle that the following table has been conducted:

		Maximum of refrigeration.	
Exper. 2.	Section of the brain.	8°	3′
Exper. 8.	Section of the spinal marrow below the 7th cervical vertebra.	8	2
Exper. 9.	Sect. in the 1st. intervertebral space of the dorsum	7	6
Exper. 10.	.... 2d	6	5
Exper. 11.	.... 3d	5	6
Exper. 12.	.... 4th	4	9
Exper. 13.	.... 5th	4	2
Exper. 14.	.... 6th	3	0
Exper. 15.	.... 7th	2	5

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* The following table shows the degree of refrigeration which had already taken place when reaction began to manifest itself:

Exper. 12.	Section in the 4th intervertebral space (dorsi)	34·7°
Exper. 13.	.... 5th	35·6
Exper 14.	.... 6th	35·7
Exper. 15.	.... 7th	38·3
Exper. 16.	.... 8th	39·0
Exper. 18.	.... 10th	39·3
Exper. 20.	.... 12th	40·5

		Maximum of refrigeration.
Exper. 16.	8th	1 9
Exper. 17.	9th	0 5
Exper. 18.	10th	1 2
Exper. 19.	11th	0 0
Exper. 20.	12th	0 6

I conclude:

1. That animal heat diminishes, in general, more slowly as the section has been performed lower down in intervertebral space. It is to be remarked that the numbers which relate to the eight superior of these spaces present a decreasing progression nearly regular.

2. The diminution being the quicker as the section paralyses a greater number of nerves, it is to this paralysis, and not to the local affection of the spinal marrow (which is always the same in every experiment) that the first phænomena must be attributed.

Now the dorsal part of the spinal marrow being in communication with no other nerves but the intercostals and the great sympathetic; as the distribution of the first, exclusively to the external part of the chest, does not permit us to suppose them the organs through which the nervous system acts on animal heat; the great sympathetic is therefore the only one which can furnish us with a probable explanation of the phænomena. It was then necessary to act upon this nerve.

For this purpose, the best process seemed to me to extract the capsula subrenalis of the left side by means of an incision below the 13th rib. As the great sympathetic nerve adheres firmly to this thin capsule, we are nearly certain of dividing it by this operation; and even in case of our failing in it sometimes, the nerve must necessarily be so much contused as greatly to debilitate its functions. Therefore, this process, how soever imperfect it appears, has been ever sufficient for furnishing decided results. In the two experiments I present, I have obtained for the average refrigeration: *

Exper. 21.—(Death 8th hour at about 27°) $\frac{39{\cdot}8^\circ - 32{\cdot}2^\circ}{4^h\,0'} = 1{\cdot}90^\circ$.

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* As to the autopsy in experiments of this kind:

1. I never observed any hæmorrhage nor inflammation; still more, I have sometimes endeavoured in such cases to produce an inflammation by injecting in the pleura or peritoneum some acrid substances, as cantharids, &c. I could never succeed to produce it, and death always happened before unequivocal signs of inflammation could be observed.

2. In most instances, the nerve is found divided; sometimes, however, this is not the case, or the *local* disorganization produced renders it impossible to determine whether it has been divided or not. It is to be observed, that the disorder in the parts situated near the great sympathetic nerve could not have any influence in the production of the preceding phænomena; for they are not observable after the extirpation of one of the kidneys: the animal recovers perfectly well.

3. A very remarkable symptom, I believe, I have observed in every case after the division of the great sympathetic nerve, is the diminution of the heart's strength. Sometimes the animal seems even to die of syncope after some hours.

Exper. 22.—(Death 10th hour at about 26°) $\frac{38{\cdot}7^\circ - 32{\cdot}5^\circ}{3^h\ 55'} = 1{\cdot}58^\circ$.

The difference between the average refrigeration in these experiments, and in those in which the brain has been acted upon, is easily accounted for by the imperfection of the process employed, which acts on one of the great sympathetic nerves only.

Being unable to destroy the action of all the branches of the great sympathetic nerves by acting on these nerves directly, it appeared to me possible to prevent this action from having any effect by depriving them of the materials on which they work. With this intention I tied the thoracic aorta immediately above its passage through the diaphragm, by means of an incision in the last intercostal space. By this operation the animal was divided in two parts; the one composed of the abdomen and hind parts entirely dead; the other comprising the thorax and head perfectly alive. These two portions of the animal cooled precisely with the same quickness, and what is worthy of notice, the dead part has always remained in an average of 0·7°, or 0·8° higher than the living one. The average refrigeration has been:

Exper. 23.—In the rectum $= \frac{38{\cdot}0^\circ - 33{\cdot}0^\circ}{2^h\ 5'} = 2{\cdot}40^\circ$. Death the fifth hour.

In the œsophagus $= \frac{37{\cdot}3^\circ - 32{\cdot}3^\circ}{2^h\ 5'} = 2{\cdot}40^\circ$. Death the fifth hour.

Exper. 24.—In the rectum $= \frac{38{\cdot}6^\circ - 34{\cdot}7^\circ}{1^h\ 25'} = 2{\cdot}74^\circ$. Death the second hour.

In the œsophagus $= \frac{37{\cdot}8^\circ - 34{\cdot}0^\circ}{1^h\ 25'} = 2{\cdot}67^\circ$. Death the second hour.

By these experiments, we find the whole average refrigeration furnished by the experiments in which the brain was acted upon restored. They terminate, for this reason, the experimental part of this inquiry. The rest of this paper presents that explanation which seems to me to be the most probable of the influence of the section of the par vagum on animal heat. But as this inquiry could be treated in an experimental manner, it seems to me useless to relate in this extract the theory I offer on this occasion.

Article VII.

Remarks on Oil and Coal Gas, in answer to Mr. Low and "A Subscriber." By M. Ricardo, Esq.

(To the Editor of the *Annals of Philosophy.*)

I must again request your insertion in the *Annals*, of a few remarks on a paper by Mr. Low, in the Philosophical Magazine of April last, which is a reply to my former statement respecting the comparative advantages of oil and coal gas. I will endeavour, for my own and your readers' sakes, to be as concise as possible, and to do no more than correct the statements, or rather misstatements, of that gentleman.

He begins by calling in question my correctness as to the quantities of light produced from oil and coal gas. In reply to his observations on this point, I need do little more than refer him to the excellent and truly philosophical paper by Dr. Henry, which was read before the Royal Society, and has since been printed. It is there shown, as was known before, by those accustomed to oil gas, that a very great difference exists in it, according to the mode in which it is prepared; how very much inferior the average of what he procured was to that which was furnished him by yourself, from an apparatus of Messrs. Taylor; and, again, how superior the latter was to the coal gas obtained from Wigan coal, which, in a former paper, he proved to be one-third better than that produced from common coal. From Dr. H.'s experiments on the combustion of these gases by oxygen, taking the average of the specimens he obtained from Messrs. Taylor, and the average of the coal gas he obtained at one hour, and at five hours, it will be found that the former contained 40 per cent. of that peculiar gas, from which these gases derive the chief of their illuminating power, and Wigan coal gas 10 per cent., and that from common coal only $6\frac{2}{3}$. Thus it is not only possible, but highly probable, that the gas which Dr. Ure tried, and whose testimony forms so triumphant a note to Mr. Low's paper, was of a quality similar to that produced by Dr. Henry, and very unlike that from Messrs. Taylor's apparatus. These gentlemen have been for many years employed in bringing their apparatus to perfection; and how far they have succeeded, no other testimony need be quoted than that to which I have referred. It is needless to remark that my experiments were tried with gas procured from a similar one, and that the results of a number of them, tried in various ways, were invariably the same. A single jet flame, issuing from an orifice of the 60th of an inch in diameter, and $1\frac{1}{4}$ inch high, gave a light equal to one mould tallow candle, six to the pound; and of six of these jets, consuming one cubic foot of gas per hour, gave a light equal to six of the above-mentioned candles. Mr. Low has *argued* upon this point, but he has shown nothing to disprove it. In estimating the quantity of gas produced from a given quantity of oil, at 100 cubic feet per gallon, I am borne out by the most correct information which I can obtain from various places where oil gas is used, where the average quantity of gas obtained rather exceeds what I have stated.

Mr. Low next endeavours to prove my statement contradictory, because I say that oil gas requires no purification, but that it is passed through a wash vessel; and he takes this opportunity of asserting, that in some places, where oil gas is used, the parties have been threatened with an indictment for a nuisance, for allowing the water through which the oil gas passes to run down the drains. Without thinking it necessary to enquire whether there is any analogy between the passing of oil gas through water, to condense any vapour that may come over with it, and the separation of sulphuretted hydrogen from coal gas, by means of lime, I would ask Mr. L. where it is that the parties have been so threatened, and from whence he could possibly have obtained this information? In contradiction to this statement, I think I may assert as a positive fact, that in no one instance whatever, where oil gas is made with a proper apparatus, that is, the apparatus made by Messrs. Taylor and Martineau, and I know of no others, does any water, which comes in contact with the oil gas, ever pass down any drain. And I am further authorized to assert a circumstance that I was not before acquainted with, that, from a late improvement in the condensing vessel, the oil gas does not pass through, or come in contact with, any water at all, until it reaches the gasometer.

I think I have pretty clearly proved that my statement that oil gas requires no purification is perfectly correct; and had Mr. Low been better acquainted with its nature, he would not have hazarded the observations he has made on this point. In the purifying of coal gas, (for no one will deny that that requires purification,) I believe that the consumers of it have daily experience that it is not *completely* effected; and there are specimens of pipes to be seen which have been so acted upon by this gas as to be almost impervious.

I should hardly have condescended to notice the wonder and astonishment which Mr. Low expresses at the quantity of gas which I have stated 1000 lights will consume, during the longest night, and the number of retorts required to produce it, but that some might suppose, from his notes of admiration and italics, that I had advanced something very absurd. On referring to my paper in the *Annals* for March last, it will be seen that, for the sake of argument, I *supposed* two establishments of oil and coal gas for 1000 lights, one consuming 2,000,000, the other 7,000,000 cube feet. I merely stated the number of lights hypothetically, applying equally to both establishments. It was a matter of no importance whether I stated 1000 or 10,000 lights, my deductions were from the quantity of gas produced and consumed. Had I stated 1200, or 1300 lights, instead of 1000, Mr. Low would have lost the opportunity of expressing so much astonishment, although the argument would have remained precisely the same; and I believe it will be found that, in estimating the quantity consumed during the longest night, I have rather under than over-rated it.

The number of retorts required for an establishment consuming 7·000·000 cube feet of coal gas annually, I have taken from Peckston's work, the best authority extant. I have made allowances for wear and tear, and repairs; but I have done that as largely in the number which I have estimated for the oil gas. As for the improvements which Mr. Low mentions, I have heard of many that have been proposed, but of none that have been *successfully* put in execution. I believe a great many have been tried, but have most generally been abandoned.

Mr. Low has stated, somewhat triumphantly, what has been done, or rather what is doing, at Derby, to prove the decided superiority of coal over oil gas. Gas is there to be furnished at the expense of the contractor for 1*s.* 8*d.* per 1000 cubic feet, and delivered into the company's gasometer; for which they are to charge 7*s.* 6*d.* to the consumer, and they expect to derive a profit of 10 per cent. Without thinking it necessary to remark upon the disproportion between the cost and selling price, I will just examine the correctness of the above statement. I have been furnished with a printed rate table of the half-yearly prices of the Derby gas light burners, and a most curious table it is. Retailing gas, from the light of a farthing candle up to a large argand burner,—the method by which the quantity of gas that passes through one of these burners is regulated, so that each consumer may have precisely his quantity and no more,—the mode on which their cocks are so constructed, that the full turn shall be the maximum,—and the exactness with which the pressure of the gasometer shall at all times, and under all circumstances, be equalised, are, I suppose, among some of those late improvements that have been alluded to. That the company mean to keep strictly to the letter of the contract on the part of the consumer, is evident from the threats they hold out of penalties, fines, and taking before magistrates; all of which must be a great recommendation to induce persons to become consumers. We will now examine the correctness of Mr. Low's statement, that gas is sold at 7*s.* 6*d.* per 1000 cubic feet. If a person contracts for a burner consuming five cubic feet per hour, till 10 o'clock, the average time of lighting will be 3[illegible] hours per night, according to the usual mode of calculating; as on the shortest day it will be from 4 to 10, = 6 hours; and on the longest day 9 to 10, = 1 hour; giving the average of 3½ hours. This, for 6 days in the week, or 312 in the year, will be 1092 hours, at 5 feet per hour; and the quantity consumed will be 5460 cubic feet in the year. For this he is charged, according to the table, 1*l.* 10*s.* 5*d.* for the half-year, or 3*l.* 0*s.* 10*d.* the whole year, or at the rate of something more than 11*s.* 1*d.* per 1000 cubic feet, instead of 7*s.* 6*d.* The above statement needs not the slightest comment; I shall, therefore, make none.

A statement of the Whitechapel-road gas light establishment now erecting, and which, with the prospectus, is lying before me, will be a sufficient answer to any observations that may be made on the amount of capital required for the erection of oil or coal gas works. The company in question give a decided preference to oil gas; they have engaged premises which, when completed and made fire-proof, will stand them in a rent of 80*l.* per annum; they have contracted for laying down upwards of five miles of mains, of various diameters; they have also contracted with the patentees for an oil gas apparatus, retorts, gasometer, iron tanks, all complete; and, allowing for contingencies, the whole fixed capital required will be about 5000*l.*; and 500*l.* more will be wanted to carry on the works, and this, I understand it is estimated, will produce about 1500·000 cube feet of gas in the year, sufficient for from 800 to 1000 lights. I believe it will be allowed that a somewhat larger capital will be required for a coal gas establishment of similar power.

To the remarks made under the signature of "A Subscriber," in the *Annals* for last month, I have only to observe, that I cannot undertake to answer individual objections on the part of particular coal gas companies. My observations on oil and coal gas are made generally, and before I admit the Subscriber's assertion, that the burners in Sheffield are superior to other burners, I must know whether other coal gas establishments are ready to admit it. The Subscriber admires my ingenious mode of calculating, but I think in that respect he very far exceeds me in ingenuity. If Mahomet will not go to the mountain, he makes the mountain come to Mahomet with a vengeance; and, instead of admitting the usual mode of calculating the number of hours of lighting, and the quantity of gas consumed, he has increased the length of time of the one, and the quantity of the other, till he has squared them exactly with his former statement, and then seems to take credit to himself for his correctness; besides, if his statement be correct, it speaks still more in favour of oil gas. There are few, I believe, who will not admit the superior brilliancy of its light; and yet, at the oldest established public oil gas works, the average quantity consumed by argand burners, and by street burners, does not exceed 1¼ cube feet each; and this calculation is made from the whole quantity consumed, and estimating the time from four o'clock on the shortest day and nine o'clock on the longest.

I here take my leave of the subject as a controversialist. My statement of the advantages of oil gas, and the observations that have been made upon it, are before the public, and if I have succeeded in drawing attention to the subject, my end is answered. I am only desirous of seeing the two fairly in competition, and then, time and experience will enable the public to form their judgment of which is preferable, and that which they find so will be most generally adopted. It has been from the strongest persuasion of the superiority of oil gas over coal gas, that I have been induced to come forward in its recommendation. Whenever I am convinced that I am in error, I will most readily acknowledge it; though, I must observe, that it must be by very different arguments from those which have been brought forward that such conviction can reach my mind.

I am, Sir, your's truly,

M. Ricardo.

Article VIII.

Extract from a Paper, entitled, "Recent Determination of the Proportions of Water, and of the Density of some Elastic Fluids." By MM. Berzelius and Dulong.*

The authors of this memoir remark, that the degree of exactitude to which chemists of the present day aspire has led them to adopt the relation between the weights of the elements of water as a standard of comparison. They then allude to the experiments of MM. Biot and Arago; and conclude, that if 1·327, the proportional number assigned by them to hydrogen, be inaccurate, it must have been derived either from an erroneous estimate of the specific gravity of hydrogen or of oxygen, or of both; observing, that the relation in volumes has this remarkable advantage, that being supported by a general law, it does not admit of any error.

The first thing to be resolved upon was, the method of obtaining hydrogen gas in purity. The authors observe that for this purpose it is not requisite to use distilled zinc, it not being preferable to the zinc of commerce. They procured the gas which they employed by acting upon zinc with sulphuric acid and water; and they observe that if it be passed through a tube containing caustic potash slightly moistened, it loses its smell completely, and is rendered perfectly pure; it may then be dried by passing it over muriate of lime. Hydrogen gas thus obtained was used in the experiments described by MM. Berzelius and Dulong. The gas was then passed in a convenient apparatus over heated oxide of copper. The water thus formed was, in some cases, received in a small vessel in a liquid state, in order that its purity might be examined. In other instances, it was passed with the excess of gas over muriate of lime. The water was found to be quite pure. The results of three experiments gave the following proportions of the gases as entering into the composition of water:

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* From Annales de Chimie et de Physique.

	Oxygen.		Hydrogen.
Exper. 1.	88·942	......	11·058
Exper. 2.	88·809	......	11·191
Exper. 3.	88·954	......	11·046

Giving a mean of

Oxygen	88·9
Hydrogen...........................	11·1
	100·0

The mean of these experiments gives 12·488 of hydrogen to 100 of oxygen, instead of 13·27, the number adopted. It is, however, to be observed, that this agrees very nearly with the previous determination of Dr. Thomson, according to which, water consists of

Oxygen....................	100·0 or	88·88
Hydrogen	12·5	11·12
		100·00

Having thus discovered an error in the usually stated composition of water, the authors proceeded to take the specific gravity of oxygen and hydrogen gases.

The authors then notice the fact observed by Mr. Dalton, that when any gas which is insoluble in water is kept over it, the gaseous contents of the water rise and contaminate the gas; and to this cause they think it probable that the error of MM. Biot and Arago may be ascribed. In order to avoid this inconvenience, MM. Berzelius and Dulong covered the surface of the water with a stratum of oil.

In taking the specific gravity of oxygen, hydrogen, azote, and carbonic acid, the method adopted was to take the weight of the exhausted flask immediately after each weighing of the full flask.

The oxygen gas used was prepared from chlorate of potash; and to separate any carbonic acid which it might contain, it was passed over a strong solution of caustic potash. The carbonic acid was obtained by nitric acid from white marble, and passed over powdered crystals of carbonate of soda before it entered the receiver. The azote was procured by decomposing ammonia by chlorine; the gas was successively passed through an acid and alkaline solution.

The authors then give the results of their experiments, and compare them with those of Biot and Arago, to which I have added the determination of Dr. Thomson.

Specific gravity of gases according to

	Berzelius and Dulong.	Biot and Arago.	Dr. Thomson.
Hydrogen	0·0688	0·07321	0·0694
Oxygen	1·1026	1·10359	1·1111
Azote	0·976	0·969	0·9722
Carb. acid	1·524	1·519	1·5277

According to these determinations, the atoms of the gases will be represented as under:

	Berzelius and Dulong.	Dr. Thomson.
Hydrogen	1·0	1
Oxygen	8·013	8
Azote	14·184	14
Carbonic acid	22·151	22

For the specific gravities of several other gases, we refer to the original paper; the experiments detailed in which appear to have been conducted with great care. We may, however, observe, that if any mixture of gases arising from the water over which MM. Biot and Arago made their experiments, rendered their hydrogen gas impure, and consequently heavier than it ought to have been, the same cause would probably have produced a similar effect upon the azotic gas. We find, however, that MM. Biot and Arago determine this gas to be lighter than it was found to be by the authors of this memoir in the proportion of 0·969 to 0·976; and as Dr. Thomson more nearly agrees with the former than the latter determination, we are inclined to consider the specific gravity of azotic gas to be nearer 0·969 than 0·976.

Article IX.

Tables of Temperature, and a Mathematical Development of the Causes and Laws of the Phænomena which have been adduced in Support of the Hypotheses of "Calorific Capacity, Latent Heat," &c. By John Herapath, Esq.

(To the Editor of the *Annals of Philosophy.*)

Cranford, Hounslow, London, June 18, 1821.

DEAR SIR,

The following tables of temperature exhibit a relation between the true temperature, according to my theory of heat, and the degrees of Fahrenheit on the air thermometer, together with the corresponding elasticities of a given volume of any kind of gas, or the corresponding volumes under a given pressure. The principal table is divided into four columns. In the first, I have given some miscellaneous observations, extracted chiefly from Murray's System of Chemistry, fourth edition, and Humbolt on Isothermal Lines, and arranged according to the degrees of Fahrenheit. Generally, however, I have not thought it necessary to interpolate the table so as to make the observations stand against the precise degree of Fahrenheit at which they have been made; but have considered it sufficient to place them against the nearest degree.

In the second column, I have given the degrees of temperature agreeably to my theory, from the beginning of the scale up to 3000. These numbers are those to which all the others are adapted. For the first 800, they are set down to every 10 degrees. From this point, which is 200 of our degrees below freezing, up to 1630, nearly 100° above the point of boiling mercury, the numbers proceed by single units, and afterwards by tens.

The third column contains numbers which show the expansive force of a given portion of gas under an invariable volume, the temperature being the same as that in the second column; or it shows the volume that a given portion of gas would assume at the temperature of the second column, supposing its elasticity to continue invariably the same. These numbers are only the squares of the numbers of the second column, with three of the right hand figures cut off for decimals. By this arrangement, we have 1000 in each column at the term of melting ice, which is a point the least, perhaps, of any other in the whole scale of temperature affected by external causes; and which, therefore, appeared to me to be the fittest for being the unity of the scale. By putting the term of melting ice 1000, we also assimilate this scale of temperature as nearly, perhaps, as convenience will allow us, to the scales more commonly in use; for supposing the elastic forces or the volumes of gas at the extreme temperatures of the fluidity of water to be as 8 to 11, there will be $172\frac{3}{5}$ths of these degrees of temperature to correspond with 180 of Fahrenheit's, which, with respect to extent, have been found to be very convenient in practice.

Another advantage seems to arise from this construction of these columns, which is, that if future observations should give a ratio in the volumes or elasticities of a given portion of gas, at the temperatures of water freezing and boiling, different from that of 8 to 11, there will be no necessity for making any difference in the arrangement or relation of these two columns; it will be only to put the boiling point a little higher or lower, as experiments may direct.

The fourth column is the temperature of the air thermometer, according to Fahrenheit, adapted to the expansion of air in the third column. It is computed from the formula $\frac{180}{375} \times (v - 1000) + 32$, in which v denotes the volume of the gas, 1000 being its volume at 32° of Fahrenheit. In these numbers I have carried the calculation to tenths of a degree only, which is as near as we can generally depend on experiments, or, perhaps, nearer. I have likewise, in the first 800 degrees, thought it sufficient to compute the Fahrenheit temperature to every hundredth of our degrees. From hence to 900, or 100 below the zero of Fahrenheit, I have calculated them to every 10th degree, thence to a few degrees above the boiling of mercury to every degree; for about 90 degrees afterwards to every 10th degree; and afterwards to every 100th.

Though I have thought it sufficient to carry the comparison between the true temperature and Fahrenheit's indications to 10ths of a degree only, yet in order that those who choose may carry it to hundredths, I have computed it at every 10th degree to hundredths, and placed the difference of the 10 degrees laterally between the two 10 degrees.

By the help of these differences, and Table I. the true temperature to 10ths of a degree, may be found, corresponding to any temperature of Fahrenheit within the limits calculated, and *vice versâ;* and by the help of the numbers under the titles of "Elasticity or Volume of Gas," and Table II. the Fahrenheit temperature corresponding to any true temperature may be taken out to any degree of accuracy, or the contrary.

TABLE I.

	8	9	10	11	12	13	14	15
1	0·8	0·9	1·0	1·1	1·2	1·3	1·4	1·5
2	1·6	1·8	2·0	2·2	2·4	2·6	2·8	3·0
3	2·4	2·7	3·0	3·3	3·6	3·9	4·2	4·5
4	3·2	3·6	4·0	4·4	4·8	5·2	5·6	6·0
5	4·0	4·5	5·0	5·5	6·0	6·5	7·0	7·5
6	4·8	5·4	6·0	6·6	7·2	7·8	8·4	9·0
7	5·6	6·3	7·0	7·7	8·4	9·1	9·8	10·5
8	6·4	7·2	8·0	8·8	9·6	10·4	11·2	12·0
9	7·2	8·1	9·0	9·9	10·8	11·7	12·6	13·5
10	8·0	9·0	10·0	11·0	12·0	13·0	14·0	15·0

TABLE II.

Increments of, or elas. of gas.	Increments of, or proportional parts of, Fahrenheit.
1	0·48
2	0·96
3	1·44
4	1·92
5	2·40
6	2·88
7	3·36
8	3·84
9	4·32
10	4·80

TABLE III.

Observations.	True temp.	Elast. or volume of gas.	Temp. Fahr. —	Observations.	True temp.	Elast. or volume of gas.	Temp. Fahr. —
Absolute cold	0	0	448·0		570	324·9	
	10	·1			80	36·4	
	20	·4			90	48·1	
	30	·9			600	60·0	275·2
	40	1·6			10	72·1	
	50	2·5			20	84·4	
	60	3·6			30	96·9	
	70	4·9			40	409 6	
	80	6·4			50	22·5	
	90	8·1			60	35·6	
	100	10·0	443·2		70	48·9	
	10	12·1			80	62·4	
	20	14·4			90	76·1	
	30	16·9			700	90·0	212·8
	40	19·6			10	504·1	
	50	22·5			20	18·4	
	60	25·6			30	32·9	
	70	28·9			40	47·6	
	80	32·4			50	62·5	
	90	36·1			60	77·6	
	200	40·0	428·8		70	92 9	
	10	44·1			80	608·4	
	20	48·4			90	24·1	
	30	52·9			800	640·0	140·8
	40	57·6			1	1·601	
	50	62·5			2	3·204	
	60	67·6			3	4·809	
	70	72·9			4	6·416	
	80	78·4			5	8·025	
	90	84·1			6	9 636	
	300	90·0	402·8		7	651·249	
	10	96·1			8	2·864	
	20	102·4			9	4·481	
	30	08 9			810	6·100	133·07
	40	15·6			11	7·721	
	50	22·5			12	9·844	
	60	29·6			13	660·969	
	70	36·9			14	2·596	
	80	44·4			15	4·225	
	90	52·1			16	5·856	
	400	60 0	371·2		17	7·489	
	10	68·1			18	9·124	
	20	76·4			19	670·761	
	30	84·9			820	2·400	125·25
	40	93·6			1	4·041	
	50	202·5			2	5·684	
	60	11·6			3	7·329	
	70	20·9			4	8·976	
	80	30·4			5	680·625	
	90	40·1			6	2·276	
	500	50·0	328·0		7	3·929	
	10	60·1			8	5·584	
	20	70·4			9	7·241	
	30	80·9			830	8·900	117·33
	40	91·6			1	690·561	
	50	302·5			2	2·224	
	60	13·6			3	3·889	

Observations.	True temp.	Elast. or volume of gas.	Temp. Fahr. —	
	834	695·556		
	5	7·225		
	6	8·896		
	7	700·569		
	8	2·244		
	9	3·921		
	840	5·600	109·31	
	1	7·281		
	2	8·964		
	3	10·649		
	4	12·336		
	5	14·025		
	6	15·716		
	7	17·409		
	8	19·104		
	9	720·801		
	850	2·500	101·20	
	1	4·201		
	2	5·904		
	3	7·609		
	4	9·316		
	5	731·025		
	6	2·736		
	7	4·449		
	8	6·164		
	9	7·881		
	860	9·600	92·99	
	1	741·321		
	2	3·044		
Greatest artificial cold yet measured.	3	4·769	90·54	
	4	6·496		
	5	8·225		
	6	9·956		
	7	751·689		
	8	3·424		
	9	5·161		
	870	6·900	84·69	
	1	8·641		
	2	760·384		
	3	2·129		
	4	5·876		
	5	5·625		
	6	7·376		
	7	9·129		
	8	770·884		
	9	2·641		
	880	4·400	76·29	
	1	6·161		
	2	7·924		
	3	9·689		
	4	781·456		
	5	3·225		
	6	4·996		
	7	6·769		
	8	8·544		
	9	790·321		
	890	2·100	67·79	
	1	3·881		
	892	795·664		
	3	7·449		
	4	9·236		
	5	801·025		
	6	2·816		
	7	4·609		
	8	6·401		
	9	8·201		
	900	810·000	59·20	
	1	11·801	8·3	
	2	13·604	7·5	
	3	15·409	6·6	
Nitrous acid freezes.	4	17·216	5·7	8·69
	5	19·025	4·9	
Ammoniacal gas condenses to a liquid.	6	820·836	4·0	
	7	2·649	3·1	
	8	4·464	2·2	
	9	6·281	1·4	
	910	8·100	50·51	
Natural temperature at Hudson's Bay.	11	9·921	49·6	
	12	831·744	8·8	
	13	3·569	7·9	
Sulphuric ether congeals	14	5·396	7·0	8·78
	15	7·225	6·1	
Nitric acid freezes specific gravity, 1·42.	16	9·056	5·2	
	17	840·889	4·4	
	18	2·724	3·5	
	19	4·561	2·6	
Liquid ammonia crystallizes	20	6·400	41·73	
	1	8·241	40·9	
	2	850·084	9·9	
Solid mercury melts.	3	1·929	9·1	
	4	3·776	8·2	
	5	5·625	7·3	8·88
	6	7·476	6·4	
	7	9·329	5·5	
	8	861·184	4·6	
	9	3·041	3·7	
	930	4·900	32·85	
	1	6·761	2·0	
	2	8·624	1·1	
	3	870·489	30·2	
	4	2·356	9·3	
	5	4·225	8·4	8·98
	6	6·096	7·5	
	7	7·969	6·6	
	8	9·844	5·7	
	9	881·721	4·8	
	940	3·600	22·87	
	1	5·481	3·0	
	2	7·364	2·1	
	3	9·249	1·2	
	4	891·136	20·3	9·07
	5	3·025	19·3	

Observations.	True temp.	Elast. or volume of gas.	Temp. Fahr. −		Observations.	True temp.	Elast. or volume of gas.	Temp. Fahr. +	
	946	894·916	18·4		Ice melts	1000	1000·000	32·0	
	7	6·809	17·5			1	1002·001	3·0	
	8	8 704	16·6			2	4·004	3·9	
	9	900·601	15·7			3	6·009	4·9	
	950	2·500	14·80			4	8·016	5·9	9·65
	1	4·401	13·9			5	1010·025	6·8	
	2	6·304	13·0			6	12·036	7·8	
	3	8·209	12·1		Mean temp. at Petersburgh	7	14·049	8·8	
Mixture 2 parts alcohol and 1 water freezes	4	910·116	11·1		Ditto at Drontheim.	8	16·064	9·7	
	5	12·025	10·2	9·17		9	18·081	40·7	
	6	13·936	9·3		Ditto at Quebec	1010	1020·100	1·65	
	7	15·849	8·4		Ditto at Stockholm.	11	2·121	2·6	
	8	17·764	7·5			12	4·144	3·6	
Mixture equal parts alcohol and water freezes	9	19 681	6·5			13	6 169	4·6	
	960	921·600	5·68		Ditto at Copenhagen.	14	8·196	5·5	
	1	3·521	9·7		Ditto at Paris	15	1030·225	6·5	
	2	5·444	3·8		Ditto at Edinburgh.	16	2·256	7·5	9·74
	3	7·369	2·8		Ditto at Dublin	17	4·289	8·4	
	4	9·296	1·9			18	6·324	9·4	
	5	931·225	−1·0	9·26		19	8·361	50·4	
Cold by equal parts snow and muriate of soda	6	3·156	·0		Ditto at London	1020	1040·400	1·39	
	7	5·089	+ ·9			1	2·441	2·4	
	8	7·024	1·8		Ditto at New York.	2	4·484	3·3	
	9	8·961	2·7		Ditto at Pekin	3	6·529	4·3	
	970	940·900	3·63			4	8·576	5·3	
	1	2·841	4·6		Ditto at Bourdeaux.	5	1050·625	6·3	
	2	4·784	5·5			6	2·676	7·3	9·84
	3	6·729	6·4			7	4·729	8·3	
Mixture 1 part alcohol and 3 parts water freezes	4	8·676	7·4		Vinous fermentation begins	8	6·784	9·2	
	5	950·625	8·3	9·36	Mean temperature at Rome	9	8·811	60·2	
	6	2·576	9·3			1030	1060·900	1·23	
	7	4·529	10·2			1	2·961	2·2	
	8	6·484	11·1			2	5·024	3·2	
	9	8·441	12·1			3	7 089	4·2	9·94
	980	960·400	12·99			4	9 156	5·2	
	1	2·361	13·9			5	1071·225	6·2	
	2	4·324	14·9			6	3·296	7·2	
	3	6·289	15·8			7	5·369	8·2	
	4	8·256	16·8	9·46		8	7·444	9·2	
	5	970·225	17·7			9	9·521	70·2	
	6	2·196	18·7			1040	1081·600	1·17	
	7	4·169	19·6		Ditto at Cairo	1	3·681	2·2	
Strong wine freezes.	8	6·144	20·6			2	5·764	3·2	
	9	8·121	1·5			3	7·849	4·2	
	990	980·100	2·45			4	9·936	5·2	
	1	2·081	3·4			5	1092·025	6·2	10·03
	2	4·064	4·4		Acetous fermentation begins	6	4·116	7·2	
Mean temp. at Fort Churchill	3	6·049	5·3			7	6·209	8·2	
	4	8·036	6·3			8	8·304	9·2	
	5	990·025	7·2	9·55		9	1100·401	80·2	
Vinegar freezes	6	2·016	8·2		Mean temp. at Cumana	1050	2·500	1·20	
	7	4·009	9·1			1	4·601	2·2	
Milk freezes	8	6·004	30·1			2	6·704	3·2	
	9	8·001	1·0			3	8·809	4·2	

Observations.	True temp.	Elast. or volume of gas.	Temp. Fahr. +	
Mean temperature at Pondicherry	1054	1110·916	85·3	10·13
	5	3·025	6·3	
	6	5·136	7·3	
	7	7·249	8·3	
	8	9 364	9·3	
	9	1121·481	90·3	
	1060	3·600	1·33	
	1	5·721	2·4	
	2	7·844	3·4	
	3	9 969	4·4	
	4	1132·096	5·4	
	5	4 225	6·4	10·22
	6	6·356	7·5	
Ether boils	7	8·489	8·5	
	8	1140·624	9·5	
Phosphorus melts	9	2·761	100·5	
	1070	4·900	1·55	
	1	7·041	2·6	
	2	9·184	3·6	
	3	1151·329	4·7	
	4	3·476	5·7	10·32
	5	5·625	6·7	
	6	7·776	7·8	
	7	9·929	8·8	
	8	1162·084	9·8	
	9	4·241	110·8	
	1080	6·400	11·87	
	1	8·561	12·9	
	2	1170·724	13·9	
	3	2·889	15·0	
	4	5·056	16·0	
	5	7·225	17·1	
	6	9·396	18 1	10·42
	7	1181·569	19·2	
	8	3·744	120·2	
	9	5·921	1·2	
	1090	8·100	2·29	
	1	1190·281	3·3	
	2	2·464	4·4	
	3	4·649	5·4	
Tallow melts	4	6·836	6·5	
	5	9 025	7·5	
	6	1201·216	8·6	10·51
	7	3·409	9·6	
	8	5·604	150·7	
	9	7·801	1·7	
Spermaceti melts	1100	1210 000	2·80	
	1	2·201	3·9	
	2	4·404	4·9	
	3	6·609	6·0	
	4	8 816	7·0	
	5	1221·025	8·1	10·61
	6	3·236	9·2	
	7	5·449	140·2	
	8	7·664	1·3	
Bees'-wax melts	9	9·881	2·3	
	1110	1232 100	3·41	
	11	4·321	4·5	
	1112	1236·544	145·5	
	13	8·769	6·6	
	14	1240·996	7·7	10·70
	15	3·225	8·8	
	16	5·456	9·8	
	17	7·689	150·9	
	18	9·924	2·0	
	19	1252·161	3·0	
	1120	4·400	4·11	
	1	6·641	5·2	
	2	8·884	6·3	
	3	1261·129	7·4	
	4	3·376	8·4	
	5	5·625	9·5	10·80
	6	7·876	160·6	
	7	1270·129	1·7	
	8	2·384	2·7	
	9	4·641	3·8	
	1130	6·900	4 91	
	1	9·161	6·0	
	2	1281·424	7·1	
	3	3·689	8·2	
	4	5·956	9·3	
	5	8·225	170·4	
	6	1290·496	1·4	10·90
	7	2·769	2·5	
Alcohol boils	8	5·041	3·6	
	9	7·321	4·7	
	1140	9 600	5·81	
	1	1301·881	6·9	
	2	4·164	8·0	
	3	6·449	9·1	
	4	8·736	180·2	
	5	1311·025	1·3	
	6	3·316	2·4	10·99
	7	5·609	3·5	
	8	7·904	4·6	
	9	1320·201	5·7	
	1150	2 500	6·80	
	1	4·801	7·9	
	2	7·104	9·0	
	3	9·369	190·1	
	4	1331·716	1·2	11·09
	5	4·025	2·3	
	6	6·336	3·5	
	7	8·649	4·6	
	8	1340·964	5·7	
	9	3·281	6·8	
	1160	5·600	7·89	
	1	7·921	9·0	
	2	1350·244	200·1	
	3	2·569	1·2	
	4	4·896	2·4	
	5	7·225	3·5	
	6	9·556	4·6	11·18
	7	1361·869	5·7	
	8	4·224	6·8	
	9	6·561	8·0	
	1170	8 900	9·07	

(*To be continued.*)

Article X.

On the Chemical Examination, Characters, and Natural History, of Arragonite, explaining also the Causes of the different Specific Gravity of its different Sub-Varieties. By Edward Daniel Clarke, LL.D. Professor of Mineralogy in the University of Cambridge.

(To the Editor of the *Annals of Philosophy.*)

SIR, *Cambridge, June* 19, 1821.

About the time that Mr. Belzoni published the interesting volume of his Travels in Egypt, he sent to me for examination a fragment of the magnificent *Soros*, which he discovered in the sepulchres of the kings of Thebes, and which has excited so much curiosity, from the description given of it by this enterprising traveller. According to his account of that *Soros*, no relique, even of Egyptian splendour, has yet been found of a more marvellous nature. It is of one integral mass, of a polished translucid stone, which he believed to be oriental alabaster,[*] covered within and without with hieroglyphics, cut in the surface of the stone, and afterwards filled with a blue pigment,[†] which yet remains in the several cavities. After a careful examination of this supposed alabaster, I had the satisfaction to inform Mr. Belzoni, that the material used by the ancient Thebans in the construction of this beautiful *Soros* was yet more remarkable than he had imagined; for that it consisted of one entire mass of arragonite. As no instance had ever occurred in the present state of our knowledge respecting arragonite, where this mineral had been before observed in equal magnitude, the opinion I had given of it was of course liable to be called in question; but it was soon afterwards confirmed by the testimony of one of the most illustrious chemists living, who considered that no doubt whatever as to the real nature of this substance could be entertained.[‡]

In the examination, however, of the stone used in the construction of the Theban *Soros*, were not the other characters decisive as to its being arragonite, its specific gravity might excite a reasonable hesitation; because, although it agree with the specific gravity given of arragonite by Brochant,[*] from Gellert; namely, 2·7; this does not correspond with Haüy's statement,[†] which makes the specific gravity of arragonite as high as 2·9. But the discrepancy, as will further appear in the sequel, admits of obvious explanation; the one being the specific gravity of stalactite arragonite; the other, namely, that of Haüy, the specific gravity of crystallized arragonite.[‡]

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[*] The terms *oriental* and *occidental alabaster* are usually applied to two distinct species of minerals The oriental, or alabaster of the ancients, is a carbonate of lime. The occidental, or alabaster of the moderns, is a sulphate of lime.

[†] This curious blue pigment is instantly reducible upon pipe-clay with a little borax to a bead of pure copper, using the common blowpipe. It is, however, insoluble in the nitric, muriatic, and nitromuriatic acids, being a frit, or glass, which contains oxide of copper.

[‡] Dr. Wollaston, to whom I transmitted a few grains only of the specimen I had received from Mr. Belzoni, immediately pronounced it to be *chaux carbonatée dure*. "The novelty," said he, "consists in the magnitude of the specimen: One is led to ask how large it may not be found? Why may there not be mountains of *arragonitic marble?*"

Previous to any additional remarks upon the nature of a substance so paradoxical as arragonite is allowed to be by all mineralogists, it will be necessary to define specifically the identical mineral to which this name is here applied. By arragonite is intended a variety of carbonate of lime distinguished from common lime spar in the following characters:

1. Superior hardness, being hard enough, in some instances, to scratch glass; but in all to make a deep incision into Iceland spar, and even to cut fluor spar; therefore called *chaux carbonatée dure*.

2. Superior specific gravity when crystallized.

3. A scopiform structure, often exhibiting diverging fibres, radiating from a common centre.

4. Small fragments rendered opaque and friable in the flame of a common candle exhibiting also a mouldering dispersion into particles by means of the common blowpipe. This mouldering dispersion into particles by means of heat is more particularly characteristic of the crystallized varieties of arragonite. It nevertheless belongs to the radiated arragonite in stalactites at the cavern of Antiparos.

5. Phosphorescence, with a green light. This property, however, cannot be considered as discriminative; because it is also possessed by some of the sub-varieties of common carbonate of lime.

6. Irreducible by fracture into the primary form of carbonate of lime. Either exhibiting rhombi more obtuse than those of common carbonate of lime; or, in some instances, disclosing no rhomboidal fracture.

As long as arragonite shall continue to offer the only anomaly in Haüy's theory of crystallization, and the cause of that anomaly shall continue to baffle the researches of chemists, so long will it be regarded as the most remarkable of mineral bodies. "The analysis of no mineral," says Prof. Cleaveland,[§] "has ever so much exercised the talents, exhausted the resources, and disappointed the expectations of the most distinguished chemists in Europe, as that of arragonite." Fortunately the appellation bestowed upon it by Werner, who first separated it as a distinct species from lime spar, being merely borrowed from the name of the Spanish province, Arragon, in which it was originally found, is not likely to convey any false ideas of its chemical nature, or to perpetuate the errors of those chemists whose ingenuity has been hitherto baffled in their endeavours to become acquainted with its constituents. Kirwan, 27 years ago,* conjectured that it contained strontian; and Prof. Stromeyer, of Gottingen, has discovered strontian in some of the sub-varieties; but it remains to be proved whether strontian be an essential, or only a casual constituent of arragonite. Mr. Holme, in a series of very accurate and elaborate experiments upon arragonite, proved that it contains a certain portion of water as essential to its chemical composition; but he was unable to detect a single atom of strontian.† In the uncertainty, therefore, still subsisting with regard to its chemical nature, it will be expedient to show how much is yet known of its natural history, and what the different appearances are which constitute its several sub-varieties.

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[*] Traité de Mineralogie, tom. i. p. 577. Paris, 1808.

[†] Traité de Mineralogie, tom. iv. (4to Edit.) p. 240. Paris, 1801.

[‡] Le caractère de la pesanteur est nécessairement soumis à quelques variations, soit à raison des différents mélanges, qui peuvent se rencontrer dans les substances qu'on examine sous ce rapport, soit à raison de la réunion plus ou moins exacte, entre elles, de toutes les molécules intégrantes qui sont entrées dans leur formation.—(Bournon, Traité, &c. vol. i. p. 14. Lond. 1809.)

[§] Treatise on Mineralogy and Geology, by Prof. Cleaveland, p. 180. Boston, 1816.

When it was first discovered, from its resemblance to chlorophane in its phosphorescence when heated, it was believed to contain fluoric acid.‡ This opinion is noticed by Baron Born in his Catalogue Raisonneé, which was published at Vienna in the year 1790; and he cites Crell's Chemical Annals for the year 1788, to show from Klaproth's analysis of arragonite, that the opinion is erroneous.§ In Baron Born's Catalogue, arragonite, for the first time, is made to class among the carbonates of lime. He calls it "*spath calcaire, prismatique, violet et blanc, â prisme hexaèdre tronqué net, des limites entre l'Arragone et Valence en Espagne.*" For a long time the insular hexagonal crystals described by De Born, and brought from Spain, were the only examples of arragonite known to mineralogists. They were usually sold at very considerable prices; sometimes as high as a guinea each; and owing to the demand, even for these,‖ the dealers in mineralogy anxiously sought, in sales, and other places, for specimens of arragonite, which they commonly denominated "hard spar;" having no other criterion than its hardness to distinguish it from common lime spar, both effervescing in acids, and both being reducible to lime by the blowpipe. One of those dealers, visiting himself the part of Spain where arragonite is found, afterwards imported into this country, a number of sub-varieties, differing in their form and structure from the large hexagonal crystals of this substance. We then became acquainted with plumose arragonite, spheroidal arragonite, and even earthy arragonite. Presently all those stalactites from the mines of Styria, and other places, which had been placed in cabinets among the carbonates of lime, under the name of *flos ferri*, because found in iron mines, being found to possess the characters of arragonite, were added to the list of the sub-varieties of this mineral under the name of *coralloidal arragonite*. To this great increase in the number of the sub-varieties of arragonite may, perhaps, be owing the observation of Brongniart, that it appeared to him to be impossible to fix any precise boundary between arragonite and the other varieties of carbonate of lime.* The best chemists are, however, unanimous in their opinions as to the propriety of distinguishing these minerals from each other. In the year 1814, our Professor of Chemistry, the celebrated Tennant, discovered arragonite among the stalactites that had been brought from the cavern of Antiparos, in Greece; and soon afterwards, it was proved by Mr. Holme, that the stalactites, from the same cavern, which Mr. Hawkins had presented to the Woodwardian collection, were also of arragonite. The remarkable discovery of Professor Tennant (while it proved, contrary to the opinion of the most eminent mineralogists and chemists of the day, that arragonite might result from a simultaneous process with that by which calcareous alabaster is deposited, and that the stalactites, both of one and of the other, might be found suspended from the roof, or investing the sides, of the same cavern), tended to throw great light upon the natural history of this mineral. It strengthens the opinion, that whatever may be the distinction between the two minerals, their difference is not of a chemical nature. It also proves, that masses of equal magnitude with any masses that have hitherto been discovered of calcareous alabaster may also reasonably be expected in arragonite. Consequently, it may be considered, as having, as it were, opened the door for the discovery made by Mr. Belzoni, as far as it affects the science of mineralogy; because, by anticipation, it established the probability that masses of arragonite equalling in magnitude even that of the Theban *Soros*, would afterwards be recognized as of genuine arragonite; and in the inspection of the substance of this *Soros*, notwithstanding its enormous size, it is evident that it was originally deposited by the stalactite process. This appears in the variety of translucid zones and layers which it exhibits, and which caused it, as a carbonate of lime, to be confounded with common calcareous or oriental alabaster. Its chemical and mineralogical characters are all of them those of arragonite; and it possesses the discriminative marks which have been before stated as proofs of the identity of this mineral. It dissolves with vehement effervescence in pure muriatic acid, without leaving any insoluble residue. When sulphuric acid is added to the solution, a disengagement of the muriatic acid immediately ensues, and the residue is a sulphate, which, as it is not wholly soluble in dilute muriatic acid, and from other trials,[*] seems to contain sulphate of strontia; but this requires further examination. Its specific gravity is 2·7. The specific gravity of the arragonite stalactites of Antiparos varies in the different specimens from 2·9 to 2·7 and 2·6, the difference wholly depending upon the degree of crystallization which the mineral has experienced. When the crystallization is perfect, the specific gravity amounts to 2·9, because those stalactites in which an incipient crystallization only is discernible, are of a more porous nature; the same degree of density does not take place in the mineral, air being admitted and held between the different fibres and layers. For this reason, the specific gravity of the solid hexagonal crystals of arragonite from Arragon equals always 2·9; but the arragonite of the Quantock Hills Cavern, in Somersetshire,[†] is exactly 2·71, as estimated both by our present Professor of Chemistry, the Rev. I. Cumming, and by myself; thereby agreeing with the specific gravity of the arragonite of the Theban *Soros*, discovered by Mr. Belzoni. The specific gravity also of the common coralloidal arragonite, or *flos ferri*, deserves to be noticed; because no account of it has yet been published; which made me the more desirous of making the experiment. For this purpose I selected a specimen from the mines of Styria, weighing $682\frac{8}{10}$ths grains. By immersion in pump water, the weight lost amounted to $250\frac{5}{10}$ths.

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* See the edition of Kirwan's Mineralogy, published in 1794.

† See Observations on Arragonite, together with its Analysis, by the Rev. John Holme, AM. FLS. as read before the Linnean Society of London, April 6, 1813.

‡ I possess a mineral exhibiting an intermediate phenomenon between common cannon spar and arragonite. It cannot be converted into lime by the blowpipe. Owing to its phosphorescence, and the resemblance of its crystalline form to apatite, it was considered in Copenhagen as a phosphate of lime. This mineral came from Greenland.

§ Catalogue Methodique et Raisonneé, &c. par M. de Born, tom. i. p. 321. Vienna, 1790.

‖ "La plupart des Cristaux isolès," says Count Bournon, "sont très-rares."—Catalogue de la Collection Mineralogique, p. 10. A Londres, 1813.

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* "Il ne nous a pas paru possible d'assigner des limites précises entre l'arragonite et les autres variétes de chaux carbonatée."—(Traité de Minéralogie, tom. i. p. 220. Paris, 1807.)

Consequently its specific gravity being 2·725 strikingly corresponds with the specific gravity of the arragonite from the Somersetshire cavern, and also with that of the remarkable *Soros* which has given rise to the preceding observations.

From all, therefore, that has been now adduced, it is plain that the same specific gravity does not apply to all the sub-varieties of this remarkable mineral; but that its specific gravity may be greater or less as crystallization is more or less advanced. Some remarks which I had made upon this subject were submitted to the Cambridge Philosophical Society soon after the fragment of Mr. Belzoni's Egyptian *Soros* arrived in Cambridge. Since that communication was made, having been occupied in the examination of some minerals that were brought by Humboldt from the Andes, I found one that had been labelled "*Quartz passing into onyx.*" Externally it resembled Cryolite so much as to deceive a very eminent mineralogist. It had been found upon Chimboraço in the kingdom of Quito at an elevation of 17,000 feet above the level of the sea. It is barely hard enough to scratch glass; but its lively effervescence in acids, added to its character before the blowpipe, soon made me acquainted with its real nature. It is in fact a very curious sub-variety of arragonite; and it has the unusual property of assuming, first, a black, and afterwards an ochreous yellow colour, before the blowpipe, owing to a portion of iron which it contains. The specific gravity also of this mineral from Chimboraço is 2·7. As it differs from all the other sub-varieties of the hard carbonate of lime in the change of colour which it sustains by the action of heat, we may venture to give it a name, founded on its locality, and call it *Chimboraçite*, although, perhaps, it may be some time before any additional specimens of it can be procured.

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[*] The insoluble sulphate, after being exposed to the action of dilute muriatic acid for the solution of the sulphate of lime, was exposed to a red heat in a platinum crucible, and again placed in dilute muriatic acid. It was then calcined at a very high temperature, and found to be soluble in distilled water; but being exposed to atmospheric air, a white pellicle was speedily formed upon the surface which fell to the bottom, another and another pellicle succeeding, until the whole substance in solution was precipitated. This precipitate tinged the flame of burning alcohol of a purple hue, and was, therefore, believed to be carbonate of strontian.

[†] About seven miles from Bridgewater.

EDWARD DANIEL CLARKE.

ARTICLE XI.

On the Carburet of Nickel. By Mr. William Ross.

(To the Editor of the *Annals of Philosophy.*)

SIR, *Manchester, June 8, 1818.*

ABOUT a year ago, being desirous of obtaining a specimen of pure nickel, I prepared some of the oxide from the impure nickel of the shops, following pretty nearly the process given by Dr. Thomson in the first volume of the fifth edition of his System of Chemistry, p. 391. The oxide I mixed with a small quantity of powdered resin, and made it into a paste with oil; it was introduced into a charcoal crucible, which was placed in sand in an earthen one; to this a cover was adapted, and secured by a lute of pipe-clay and sand. In a day or two, when the luting had become dry, the crucible with its contents was placed in a powerful forge, and exposed to the most violent heat I could raise for about three-quarters of an hour. By this process, I expected to have a compact bright button of nickel: however, I only obtained a piece of imperfectly fused metal, having the aspect of plumbago, and soiling the fingers precisely as that substance does. I was desirous to get rid of this plumbago-like substance, a considerable portion of which was disseminated through the whole mass, and thought, that by again exposing it to a strong heat, in a clean earthen crucible, without any charcoal, when it was melted, the substance would rise to the surface. I, therefore, put it a second time into the forge without any admixture of carbonaceous matter, and kept it in an intense heat for some time; however, the effect was not what I had anticipated;—the piece of metal was certainly somewhat denser, and a portion of the substance had come to the surface of it, giving it the appearance of being coated with iodine, or micaceous iron ore, but it was so deficient in ductility, that I could not hammer it into a plate beyond 1-10th of an inch in thickness without cracking. I was discouraged in my undertaking, and gave up the idea of possessing a specimen of pure malleable nickel. The piece of metal I wrapped up in a bit of paper, and placed it among my other chemical substances. I had almost forgotten it, when, a short time ago, in looking for some other substance, it accidentally attracted my attention. On examining it with a little reflection, it struck me that this plumbago-like substance was a true compound of carbon and nickel, or a carburet of nickel. No such compound has ever been described, I believe, in any of our chemical treatises; and indeed I am not aware any of our chemists have said a word as to its existence, except Dr. Thomson, who does just observe, that a combination of this nature has been hinted at, but, if I recollect right, gives it as his opinion that a real carburet of nickel has not hitherto been discovered.

Within these few days, I have perused M. Tupputi's memoir upon nickel (see Annales de Chimie, tome lxxviii): he plainly intimates that this metal is capable of uniting with carbon. It seems rather singular, that the French chemist's observation should have met with so little attention; and as the circumstance of my meeting with this curious substance appears to verify his remark, I have ventured (though with no little diffidence I assure you, Sir, for I am quite a young chemist) to address you on the subject.

I inclose you a small quantity of the substance to operate upon, if you think proper, and to discover whether I am correct in my conjecture. It has been pulverized. I procured it by exposing the whole mass to the action of dilute nitric acid; the metal was dissolved, and it remained at the bottom of the vessel unaltered.

I am, Sir, yours, with the greatest respect,

William Ross.

ARTICLE XII.

ANALYSES OF BOOKS.

A Geological Classification of Rocks, with descriptive Synopses of the Species and Varieties, comprising the Elements of Practical Geology. By John Macculloch, MD. FRS. FLS. Vice Pres. Geol. Soc. &c. &c. &c.

IT is probably known to our geological readers, that, excepting some very imperfect attempts towards a work of this nature, no classification and description of rocks has yet been published. The want of one has been much felt by every geological student, and the present publication is intended to supply it.

Dr. Macculloch has adopted a geological plan for the classification of rocks, and defends the superior advantages of it in an argumentative chapter of some length at the commencement of his book. Our readers are probably aware that the celebrated Brongniart had some time ago adopted a plan founded on the mineral characters of rocks. Dr. M.'s plan is, therefore, similar to that of Werner, but he has made many important variations in the arrangement, as well as some considerable additions to the families of rocks. The principal difference in his method of arrangement will be found in the divisions of unstratified rocks which he has given, and which those who have read his work on the Western Islands of Scotland would naturally have expected to find.

A number of preliminary chapters are occupied in general details respecting the natural history and characters of rocks; and some space is also occupied in a plan for facilitating the student's labours by collecting their general mineral characters in certain groups, and in an account of the characters of the classes into which rocks have been divided.

The remainder of this work is occupied in the descriptions of the families, and of all the varieties appertaining to each, which the author seems to have thought requiring description, or deserving of it. To give our readers a general notion of the author's views respecting their families, or groups of rocks, we subjoin his tabular view:

PRIMARY CLASS.

Unstratified.

Granite.

Stratified.

Gneiss,
Micaceous schist,
Chlorite schist,
Talcose schist,

Primary Class.

(*Stratified continued.*)

Hornblende schist,
Actinolite schist
Quartz rock,
Red sandstone,
Argillaceous schist,
Diallage rock,
Limestone,
Serpentine,
Compact felspar.

Secondary Class.

Stratified.

Lowest (red) sandstone,
Superior sandstone,
Limestone,
Shale.

Unstratified.

Overlying (and venous) rocks,
Pitchstone.

Occasional Rocks.

Jasper,
Siliceous schist,
Chert,
Gypsum,
Conglomerate rocks,
Veinstones.

Appendix.

Volcanic rocks,
Clay, marle, sand,
Coal,
Alluvia,
Lignite,
Peat.

For the purpose of describing these different families consistently with his plan, a geological preface, a history of the relations and general geological characters of each, is given. This part of the work contains the elements of geology, as far as it relates to rocks; those parts which relate to actions and events, and to geological causes or theories, being reserved for a general system of geological science, which the author, in his preface, insinuates to be nearly ready for publication.

The geological history of each family is followed by a synoptical table of all the varieties or species usually included under it, in which the author has introduced minute descriptions of all those which he professes to have thought deserving of notice, and within his reach. These descriptions are of such a nature as to enable a student to verify such specimens as he may have to examine, and to refer them to their generic place, or to that which they hold among the families here adopted.

A specimen of this work; namely, that part which belongs to gneis, will be found in his work on the Western Islands; and as our limits will not now admit of a more full detail of his plan and its execution, we shall defer it to some future number; but in the mean time recommending this work as highly worthy of the notice and examination of geologists.

II. *Journal of a Voyage for the Discovery of a North-west Passage from the Atlantic to the Pacific, performed in the Years 1819, 1820, in his Majesty's Ships Hecla and Griper, under the Orders of W. E. Parry, R.N.*

IT would be inconsistent with our plan to enter at any length into an analysis of this interesting work; but there are some parts of it more particularly connected with scientific objects which we have thought would be acceptable to the reader to see briefly stated as notices. It is but justice to observe that Capt. Parry's account of his arduous and perilous undertaking is written in a clear and manly style.

On the Variation of the Magnetic Needle.

Capt. Parry observed that, from the time he first entered Sir James Lancaster's Sound, the sluggishness of the compasses, as well as the amount of their irregularity produced by the attraction of the ship's iron, had been found very rapidly, though uniformly, to increase as he proceeded to the westward: this irregularity became more and more obvious as he advanced to the southward. The rough magnetic bearing of the sun at noon, or at midnight, or when on the prime vertical, as compared with its true azimuth, was sufficient to render this increasing inefficiency of the compass quite apparent.

It was, therefore, evident, that a very material change had taken place in the dip, or the variation, or in both these phenomena, which rendered it not improbable that he was making a very near approach to the magnetic pole. He afterwards witnessed the curious phenomenon of the directive power of the needle becoming so weak as to be completely overcome by the attraction of the ship; so that the needle might now be said to point to the north pole of the ship. It was only, however, in those compasses in which the lightness of the cards, and great delicacy in the suspension, had been particularly attended to, that even this degree of uniformity prevailed; for, in the heavier cards, the friction upon the points of suspension was much too great to be overcome even by the ship's attraction, and they consequently remained indifferently in any position in which they happened to be placed.

Captain Sabine afterwards observed when on shore at Prince Regent's Inlet on Aug. 7, for the purpose of making magnetic observations, that the directive power of the horizontal needle, undisturbed as it was by the attraction of the ship, was even here found to be so weak in his azimuth compasses, which were the most sensible, that they required constant tapping with the hand to make them traverse at all.

At Martin's Island on Aug. 28, the dip of the magnetic needle was 88° 25′ 58″, and the variation was now found to have changed from 128° 58′ W. in the longitude of 91° 48′, where the last observations on shore had been made, to 165° 50′ 09″ E. at this station, so that in sailing over the space included between these two meridians, they crossed immediately to the northward of the magnetic pole over one of those spots upon the globe, where the needle would have been found to vary to 180°; or, in other words, where its north pole would have pointed due south. This spot would, in all probability, at this time be some where not far from the meridian of 100° W. of Greenwich. Capt. Sabine remarked, in obtaining the observations for the variation, that the compasses, which were those of Capt. Kater's construction, required somewhat more tapping with the hand to make them traverse than they did at the place of observation in Prince Regent's Inlet on August 7, where the magnetic dip was very nearly the same; but that, when they had settled, they indicated the meridian with more precision.

The azimuth compasses used in these observations were of Capt. Kater's improved construction. These compasses were originally constructed for the voyage of discovery in 1818, and are described in the Philosophical Transactions for 1819, a further improvement having been made in them during the equipment of this expedition.

In the course of these observations, two objects were designed to be kept in view; it was requisite, first, for the purposes of navigation, that the amount of the variation on the courses steered by the ship should be known, that her true direction might be deduced from that indicated by the compass; and, secondly, to these necessary observations, it was desired to add such as could be made without material inconvenience or delay with the ship's head placed on other points than those of her immediate courses, for the purposes of exemplifying more extensively than had been done heretofore the irregularities which take place in the direction of compass needles in consequence of the attraction of the iron contained in ships.

It was evident from a course of experiments that the common centre of attraction of the ship's iron was forward and very nearly a-midships; and that, consequently, when the ship's head was north or south by the compass, the direction of the earth's magnetism and of the local attraction coinciding, the compass indicated the true magnetic bearing of objects.

The true variation of the needle, therefore, could be at all times ascertained by azimuths observed with the ship's head, on either of those points; when the error with which the result might be affected from local attraction might be reasonably expected not to exceed the other incidental errors to which such observations are necessarily liable. The irregularities in the direction of the compass proceeding from the ship's iron occasioned no other practical inconvenience in her navigation than a little additional trouble in computing the day's works.

An Account of the Experiments to determine the Acceleration of the Pendulum in different Latitudes.

The nature of these experiments may be briefly described to consist in ascertaining with the utmost possible accuracy at different stations, the latitudes of which are correctly known, the number of vibrations which would be made by a certain pendulum in a given time, were it placed at the level of the sea, in vacuo, and at a certain temperature. This purpose is effected by setting up a clock containing the pendulum in a convenient and protected situation, and by observing the number of vibrations which it makes on an average of several intervals of 24 hours each, accurately determined; the actual circumstances of the temperature, pressure of the atmosphere, arcs in which the vibrations are performed, and the elevation above the sea, being carefully noted, and their effects in retarding or accelerating the vibrations calculated and allowed for. This operation, which is sufficiently simple in description, proves less so in the performance, by reason of the extreme accuracy which is required in the results, and of the many causes whereby slight errors may be introduced, which demand the utmost precaution and watchfulness to guard against.

The squares of the number of vibrations in 24 hours in the different latitudes are to each other as the force of gravitation in such latitudes; and the difference between the polar and equatorial diameters is deduced from the acceleration obtained by comparing the observations at each station successively with those at all the others.

Two clocks were used in these experiments, being the property of the Royal Society, and the same which accompanied Capt. Cook round the world. The pendulums were prepared by Capt. Kater, being each cast in one piece of solid brass, and vibrating on a knife-edge of hard steel, on agate plates ground into portions of hollow cylinders.

The experiments were made in the present voyage, and in the preceding one in 1818; one only of the clocks was employed on the first voyage, but both in the second.

The stations at which the experiments in 1818 were made are as follow:

At Brassa, one of the Shetland Islands, lat.	60°	09′	42″	0‴	N.
At Hare, or Waygat Island, in Davis's Strait, lat.	70	26	15	0	N.
At London, lat.	51	31	08	4	N.

The stations of the present voyage were:

In London, latitude as before.
At Melville Island, in the Polar Sea, lat. ... 74 47 14 36 N.
And again in London on the return of the expedition.

The observations of the second voyage are deserving of principal consideration for the following reasons:

1. The arc of intercepted latitude was greater than between any two of the stations of 1818; the possible errors of observation have consequently had a less influence on the accuracy of the deductions.

2. The employment of two clocks afforded a means of procuring double and corresponding results. Indeed as it happened fortunately that each pendulum would fit into either clock, four corresponding results were obtained for the acceleration between London and Melville Island.

3. The number of vibrations made by the clocks in London was ascertained by two distinct series of experiments, one before the departure, and a second after the return of the expedition; the very near agreement of the results on these occasions proving that neither of the clocks, nor any part of their apparatus, had sustained any injury during the voyage, affecting their rate of going; as well as affording a satisfactory inference of the confidence which is due to this mode of experiment; the number of vibrations in 24 hours in London was as follows:

Vibrations in a mean solar day.

Clock 1.	January,	1819	86392·5673	86392·4513
	December,	1820	86392·3353	
Clock 2.	March,	1819	86496·997	86496·9855
	December,	1820	86496·9741	

4. From the time which was allowed for the experiments at Melville Island being fully sufficient for their completion, the rate of each clock being determined by a mean of 85 intervals of 24 hours each.

And, lastly, from the correspondence in the results obtained by the two clocks; the daily acceleration of the one, on a mean of the experiments with both pendulums, being 74·8151 vibrations, and by the other, 74·6528 vibrations. The mean of the two, therefore, 74·734, is considered as the true acceleration of a pendulum between the latitudes of 51° 31′ 08″ 4‴ and 74° 47′ 14″ 36‴ N.

By comparing, in a similar manner, the experiments made in the voyage of 1818 with each other, the acceleration of the pendulum between the several stations at which it was tried, has been obtained as follows:

Vibrations in a mean solar day.

Between London and Brassa	33·107
London and Hare Island.	65·2386
Brassa and Hare Island	32·1316

The following table contains the deductions which have been obtained by calculation from these several results:

From the acceleration between	Diminution of gravity from the pole to the equator.	Ellipticity of the earth.
London and Braśsa	·0055066	$\frac{1}{314\cdot3}$
London and Hare Island	·0055139	$\frac{1}{315\cdot6}$
Brassa and Hare Island	·0055082	$\frac{1}{314\cdot4}$
London and Melville Island	·0055258	$\frac{1}{318\cdot6}$

Aurora Borealis.

On the evening of January 15, Capt. Parry saw the only very brilliant and diversified display of Aurora Borealis which occurred during the whole winter, of which Capt. Sabine has given the following account:

"Mr. Edwards, from whom we first heard that the Aurora was visible, described it as forming a complete arch, having its legs nearly north and south of each other, and passing a little to the eastward of the zenith. When I went upon the ice, the arch had broken up; towards the southern horizon was the ordinary Aurora, such as we had lately seen on clear nights, being a pale light, apparently issuing from behind an obscure cloud, at from six to twelve degrees of altitude, extending more or less towards the east or west on different nights, and at different times of the same night, having no determined centre or point of bisection, the greater part, and even at times the whole of the luminous appearance being sometimes to the east, and sometimes to the west of south, but rarely seen in the northern horizon, or beyond the east and west points of the heavens. This corresponds with the Aurora most commonly noticed in Britain, except that it is there as peculiar to the northern as here to the southern horizon, occasionally shooting upwards in rays and gleams of light. It was not distinguished by any unusual brilliancy or extent on this occasion, the splendid part of the phenomenon being detached, and apparently quite distinct.

"The luminous arch had broken into irregular masses, streaming with much rapidity in different directions, varying continually in shape and intensity, and extending themselves from N by E to S. If the surface of the heavens be supposed to be divided by a plane passing through the meridian, the aurora was confined, during the time I saw it, to the eastern side of the plane, and was usually most vivid, and in larger masses, in the ESE than elsewhere. Mr. Parry and I noticed to each other that where the aurora was very brilliant, the stars seen through it were somewhat dimmed, though this remark is contrary to former experience.

"The distribution of light has been described as irregular and in constant change: the various masses, however, seemed to have a tendency to arrange themselves into two arches, one passing near the zenith, and a second about midway, between the zenith and horizon, both having generally a north and south direction, but curving towards each other, so that their legs produced would complete an ellipse; these arches were as quickly dispersed as formed. At one time a part of the arch near the zenith was bent into convolutions, resembling those of a snake in motion, and undulating rapidly; an appearance which we had not before observed. The end towards the north was also bent like a shepherd's crook, which is not uncommon. It is difficult to compare the light produced by an Aurora with that of the moon, because the shadows are rendered faint and indistinct by reason of the general diffusion of the aurora; but I should think the effect of the one now described scarcely equal to that of the moon when a week old. The usual pale light of the aurora strongly resembles that produced by the combustion of phosphorus; a very slight tinge of red was noticed on this occasion, when the aurora was most vivid, but no other colours were visible. Soon after we returned on board, the splendid part wholly disappeared, leaving only the ordinary light near the horizon; in other respects, the night remained unchanged, but on the following day, it blew a fresh gale from the N and NNW. This aurora had the appearance of being very near us, and we listened attentively for the sound which is said sometimes to accompany brilliant displays of this phenomenon; but neither on this nor on any other occasion could any be distinguished."

No effect was produced by the aurora on the electrometer, or the magnetic needle.

Effects of Cold as observed by Capt. Parry at Melville Island.

"On Oct. 29, the weather was calm and clear, and we remarked for the first time, that the smoke from the funnels scarcely rose at all, but skimmed nearly horizontally along the housing, the thermometer having got down to — 24°, and the mercury in the barometer standing at 29·70 inches. It now became rather a painful experiment to touch any metallic substance in the open air with the naked hand; the feeling produced by it exactly resembling that occasioned by the opposite extreme of intense heat, and taking off the skin from the part affected. We found it necessary, therefore, to use great caution in handling our sextants and other instruments, particularly the eye pieces of the telescopes, which, if suffered to touch the face, occasioned an intense burning pain; but this was easily remedied by covering them over with soft leather. Another effect, with regard to the use of instruments, began to appear about this time. Whenever any instrument which had been some time exposed to the atmosphere, so as to be cooled down to the same temperature, was suddenly brought below into the cabins, the vapour was instantly condensed all around it, so as to give the instrument the appearance of smoking, and the glasses were covered almost instantaneously with a thin coating of ice, the removal of which required great caution to prevent the risk of injuring them until it had gradually thawed, as they acquired the temperature of the cabin. When a candle was placed in a certain direction from the instrument, with respect to the observer, a number of very minute *spiculæ* of snow were also seen sparkling around the instrument at the distance of two or three inches from it, occasioned, as we supposed, by the cold atmosphere produced by the low temperature of the instrument almost instantaneously congealing into that form the vapour which floated in its immediate neighbourhood."

At noon on Jan. 11, the temperature of the atmosphere had got down to 49° below zero, being nearly the greatest degree of cold Capt. Parry experienced; but the weather being quite calm, he continues, "we walked on shore for an hour without inconvenience, the sensation of cold depending much more on the degree of wind at the time than on the absolute temperature of the atmosphere, as indicated by the thermometer. In several of the accounts given of those countries in which an intense degree of natural cold is experienced, some effects are attributed to it which certainly did not come under our observation in the course of this winter. The first of these is the dreadful sensation said to be produced on the lungs, causing them to feel as if torn asunder, when the air is inhaled at a very low temperature. No such sensation was ever experienced by us, though in going from the cabins to the open air, and *vice versâ*, we were constantly in the habit, for some months, of undergoing a change of from 80° to 100°, and, in several instances, 120° of temperature in less than one minute; and what is still more extraordinary, not a single inflammatory complaint, beyond a slight cold which was cured by common care in a day or two, occurred during this particular period. The second is, the vapour with which the air of an inhabited room is charged condensing into a shower of snow, immediately on the opening of a door or window communicating with the external atmosphere. This goes much beyond any thing we had an opportunity of observing. What happened with us was simply this: on the opening of the doors at the top and bottom of our hatchway ladders, the vapour was immediately condensed by the sudden admission of the cold air, into a visible form, exactly resembling a very thick smoke, which settled on all the pannels of the doors and bulk-heads, and immediately froze, by which means the latter were covered with a thick coating of ice which it was frequently necessary to scrape off; but we never, to my knowledge, witnessed the conversion of the vapour into snow during its fall."

Capt. Parry remarks, that the lower rigging of the ship became very slack during the severity of the winter, and again became tight as the warmer weather came on. He mentions this fact, because the circumstance of its becoming slack by the cold is at variance with the accounts of other navigators.

On Feb. 14 and 15, for 15¼ hours, during which time the weather was clear and calm, the thermometer fixed on a pole between the ships and the shore, never rose above − 54°, and was once during that interval, namely, at six in the morning, as low as − 55°. Some mercury was frozen during the continuance of this cold weather, and beaten out on an anvil, previously reduced to the temperature of the atmosphere; it did not appear to be very malleable in this state, usually breaking after two or three blows from the hammer.

By filling small shallow boxes with mould, and placing them along the stove pipe in the cabin, even in the severity of the winter, Capt. Parry found he could generally raise a small quantity of mustard and cress, in six or seven days, though the fire was not kept in at night. The mustard and cress thus raised were necessarily colourless, from the privation of light, but as far as he could judge, they possessed the same pungent aromatic taste as if grown under ordinary circumstances.

The distance at which sounds were heard in the open air during the continuance of intense cold was so great as constantly to afford matter of surprise to him, notwithstanding the frequency with which he had occasion to remark it. People might often be heard distinctly conversing, in a common tone of voice, at the distance of a mile. Lieutenant Beechy and Messrs. Beverly and Fisher, in the course of a walk which led them to a part of the harbour about two miles directly to leeward of the ships, were surprised by suddenly perceiving a smell of smoke, so strong as even to impede their breathing, till, by walking on a little further, they got rid of it. This circumstance shows to what a distance the smoke from the ships was carried horizontally, owing to the difficulty with which it rises at a very low temperature of the atmosphere.

Article XIII.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

May 31.—The reading of Mr. Herapath's paper, on the Absolute Zero, was concluded.

The object of Mr. Herapath has been to determine the *law of temperature*, and the point of absolute cold. For this purpose, he contrived an apparatus for obviating the effects of radiation; and, having mixed equal weights of mercury at a very high and low temperature, he carefully ascertained the temperature of this mixture.

In seven experiments of his own, thus made, and two of Mr. De Luc's, he found that the results followed a law from which they differed, at a medium, not more than 1-10th of a degree.

This law is, that the square of the temperature of a given portion of gas varies as the elasticity and volume conjointly; and, therefore, when either continues the same, the temperature is as the square root of the other.

Hence Mr. Herapath finds, that the heat of boiling water is to that of melting ice as $\surd 11$ to $\surd 8$, or as 1·1726 to 1 nearly; and the point of absolute cold he also determines in a manner independent of any theory of heat, from the principle of an air thermometer.

These experiments agree precisely with a theory given by Mr. Herapath in the paper which he has just published in the *Annals*, on the Causes, Laws, &c. of Heat, Gases, and Gravitation, written many months before he undertook the experiments in question.

June 7.—On the Remeasurement of Sir J. Shuckburgh's Cube, Cylinder, and Sphere, by Capt. Kater.

June 21.—On the Parallax of the Fixed Stars, by Dr. Brinkly.

GEOLOGICAL SOCIETY.

March 16.—The reading of Mr. Strangway's paper on the Geology of Russia was continued.

From the Baltic Isles, through Esthonia, the north of Livonia and Ingria, up to Vitegra, at the south-east corner of Lake Onega, a strong and regular series of secondary rocks is observable. The lowest is a pale greenish blue clay, which is surmounted by sand or sandstone, alternating in the neighbourhood of Petersburg, with beds of shale. Above this a thick bed of limestone occurs, which is characterised by the same fossils that mark the limestone of Sweden and Norway, according to Von Buch.

The northern salt district stretches in a line parallel to the Petersburg limestone above-mentioned for 1000 versts, and makes its first appearance in the isle of Osel. It is worked in some parts of Livonia. Gypsum is also quarried in some places, and exported in large quantities to Petersburg from Riga. It is capable of a high polish, and much resembles the oriental alabaster; but true alabaster has only as yet been found at Osel.

On the south side, the valley of Novgorod is bounded by the ridge of the Valday hills, which, though they form the principal chain between the Baltic and the Black Sea, yet do not exceed 800 or 900 feet in height. Their escarpment is greatest towards the north. All the chief rivers of Russia rise from them. Near the town of Boravichy, which is situated at the foot of the Valday hills, are the falls or rapids of Nista; and as the river, for nine or ten miles above the town, runs between lofty and precipitous cliffs, excellent sections of the strata are exposed. Where the first ridge of hills abuts against the stream, is a lofty perpendicular cliff, of which the upper part consists of a pale reddish sand, presenting patches of a bright yellow colour towards the bottom: below is a black sandy clay, mingled with pyrites, and connected with a rugged ironstone, which contains charred wood. About six versts above the town of Borovichy is the lower fall, where the sand appears in horizontal strata, containing thick and regular beds of an argillaceous ironstone, which is an agglomerate of charred wood and every sort of geode, resembling the sandstone of the Popovca, and other streams, near Petersburg, in its superficial appearance. Above is a reddish sand, and below is a yellow sand, which presents a pseudo stratified structure. The next beds are blue limestone, which continues to near the water's edge, and contains madrepores, resembling those of the mountain limestone in Northumberland, and a very peculiar sort of Briarean Encrinite, the joints of which are extremely minute. Impressions of large tufts of this encrinite cover the greater part of the surface of these limestone strata, bending sometimes in one direction, and sometimes in another. They are also found in the yellow and more sandy variety of this limestone, which is discovered within the bed of the river. The latter also contains fragments of large encrini, corallines, and other marine fossils.

At the upper fall, though the banks of the river are not so high as at the lower, yet good sections of the limestone are presented. It contains chert *in situ*, and both the limestone and chert are varied with yellow and red patches, and pass gradually into each other, exhibiting several fossils similar to those abovementioned. Below these beds, the pyrites and coal shale reappear at the water's edge.

The Nista, though in many places very shallow, and obstructed by rocks, is of great importance in the internal navigation of Russia, as it cuts through almost the whole breadth of the Valday hills, and is joined by a canal to the Tvertza, which rises not far from the source of the former, and flows southward, by which means a communication between the Baltic and Caspian seas has been effected.

A paper was read, entitled, "Notice of the Discovery of a New Fossil Animal, forming a link between the Ichthyosaurus and Crocodile, together with general Remarks on the Osteology of the Ichthyosaurus. From the Observations of H. T. De la Beche, Esq. F. R. S. and M. G. S. and the Rev. W. D. Conybeare, F. R. S. and M. G. S. Communicated by the latter."

This memoir contains a notice of the discovery of the remains of an entirely new animal, allied to the order Lacerta, among the fossil bones imbedded in the lias, to which the name of Plesiosaurus has been assigned. This animal is highly interesting, as exhibiting in its structure a link between the existing genera of the above order, and the very remarkable genus Ichthyosaurus, or Proteasaurus, between which and the genus Crocodile, it would occupy an intermediate place in a natural arrangement. The head bones of this animal yet remain to be discovered. The vertebræ nearly agree with those of the crocodile, in their general form, in the arrangement of their various processes, and in the suture by which their annular part is attached to the body; they differ, however, in having narrower proportions, in their articulating surfaces being slightly concave on both sides, instead of concave on the one and convex on the other, in bearing the greater number of the dorsal ribs on a single articulating face at the end of their transverse processes, and in the number of dorsal vertebræ, which greatly exceeds that in the crocodile.

The bones of the anterior extremities are the most interesting, as affording double analogies to the Ichthyosaurus on the one hand, and existing reptiles on the other. The Plesiosaurus exactly agrees with the former in the broad, flat, and hatchet-shaped clavicles which distinguish it, but has a distinct humerus, radius, and ulna, in place of the single bone, which, in the Ichthyosaurus, supplies the place of these three. The paddle forms a very curious link between that of the Ichthyosaurus and sea-turtle; agreeing with the former in the number of its joints, and the rounded form of the external series of them, but with the latter in the form of all the internal series of phalanges.

This paper also contains general remarks on the order of fossil remains to which this genus is allied, and, in particular, a regular osteological description of the Ichthyosaurus, the most original and interesting feature of which is, a minute examination of the osteology of the head of that animal; in which the existence of all the bones which characterize that part in the lacerta tribe generally, and in the crocodile in particular, is demonstrated by a series of careful dissections, and the true place and relations of this animal thus satisfactorily ascertained.

The principal deviations from the type of the crocodile are found to be such only as naturally arise from the more elongated form of the whole head, and from the bones being applied to each other with a squamous and overlapping suture; the latter structure, which combines flexibility with strength, being probably designed to fit it for residence in the waters of the ocean, as the structure of its vertebral column appears to have been to facilitate its motions in that element. The method of M. Cuvier is strictly followed throughout these descriptions.

Article XIV.

SCIENTIFIC INTELLIGENCE, AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Succinic Acid.*

M. Julin has favoured the editor with the following notice:

The distillation of this acid from amber in the general way produces it in a very small quantity; by coarsely powdering the amber, and then mixing to it previously to the distillation 1-12th part sulphuric acid diluted with equal weight of water, the succinic acid will be produced in about twice the quantity got in the old way. The acid is easily purified by crystallization from sulphurous and sulphuric acid with which it will be found contaminated. I believe the adding of sulphuric acid in order to increase the production of succinic acid was first noticed in a late German paper: I have forgotten by whom: it was there advised that the diluted acid, after being mixed with the amber, should be evaporated; this makes the process tedious, and is not necessary, as several experiments have convinced me; the distillation of the mixture, without any previous evaporation, will be found more advantageous. I am not aware of this circumstance being noticed in any English publication; it may, therefore, be acceptable to your readers.

II. *Effects of Copper on Vegetation.*

Some time since I accidentally spilt some solution and oxide of copper near the root of a young poplar tree. In a short time, the tree began to droop, the leaves on the lower branches dying first, and eventually those on the upper ones. On cutting a branch from the tree, I observed that the knife was covered with copper to the whole breadth of the branch, showing that the copper had been absorbed, and had undoubtedly proved fatal to the life of the tree. I am not aware whether this circumstance has been before remarked.—*Ed.*

III. *Ignition occasioned by Sulphuric Acid and Barytes.*

Mr. Barry informs me that when concentrated sulphuric acid is poured upon caustic barytes, ignition ensues. This circumstance was first noticed on using the acid to determine whether the nitric acid had been perfectly expelled from the nitrate in preparing the caustic barytes. It has been already noticed that light is evolved when sulphuric acid is poured upon lime or magnesia, but I do not know that ignition has been observed similar to that which I have now described. —*Ed.*

ARTICLE XV.

NEW SCIENTIFIC BOOKS

PREPARING FOR PUBLICATION.

Dr. Adam Dods will shortly publish the Physician's Guide, being a popular Dissertation on Fevers, Inflammations, and all Diseases connected with them.

Mr. S. F. Gray has in the press, in two octavo volumes, a Natural Arrangement of British Plants, preceded by an Introduction to Botany.

JUST PUBLISHED.

A Syndesmological Chart, or a Table of the Ligaments of the Human Skeleton, by J. Dickinson, MD. 1*s*.

The History of the Plague, as it has lately appeared in the Islands of Malta, Gozo, Corfu, Cephalonia, &c. detailing important Facts illustrative of the specific Contagion of that Disease, with Particulars of the Means adopted for its Eradication. By J. D. Tully, Esq. 8vo. 10*s*. 6*d*.

Essays on Hypochondriasis, and other Nervous Affections. By John Reid, MD. 8vo. 12*s.*

An Account of the Rise, Progress, and Decline of the Fever, lately epidemical in Ireland, together with Communications from Physicians in the Provinces, and various official Documents. By F. Barker, MD. and J. Cheyne, MD. 2 Vols. 8vo. 1*l.* 6*s.*

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A Manual of Mineralogy; containing an Account of Simple Minerals, and also a Description and Arrangement of Mountain Rocks. By Robert Jameson. 8vo. 15*s.*

A Selection of the Correspondence of Linnæus, and other Naturalists, from the Original Manuscripts. By Sir James Edward Smith, MD. 2 Vols. 8vo. 1*l.* 10*s.*

Elements of the Philosophy of Plants: containing the Scientific Principles of Botany; Nomenclature, Theory of Classification, Phytography, Anatomy, Chemistry, Physiology, Ecography, and Diseases of Plants: with a History of the Science, and Practical Illustrations. By A. P. Decandolle and K. Sprengell. Translated from the German. 8vo. 15*s.*

General and Particular Descriptions of the Vertebrated Animals, arranged conformably to the Modern Discoveries and Improvements in Zoology. By Edward Griffith. Part I. containing Order Quadrumana. 35 coloured Plates, 4to. 1*l.* 5*s.*

Article XVI.

NEW PATENTS.

James Henry Marsh, of Chenies-street, Tottenham-court-road, for improvements on wheeled carriages.—April 17, 1821.

James Smith, of Hackney, for an improvement or improvements in the method or methods of machinery employed for shearing or cropping woollen cloth.—April 18.

Alexander Law, of the Commercial-road, Stepney, founder, for an improved mode of forming bolts and rails for ship fastenings, &c.—May 1.

William Thomas and Joseph Lobb, of Sithney, Cornwall, for a machine for cutting and preparing lay or lea ground for tillage with less expence, and in a shorter time than by the present mode of ploughing; and also for renewing grass land, lay or lea ground, with seeds, without destroying the surface.—May 1.

Robert Delap, of Belfast, Ireland, merchant, for improvements in producing rotatory motion.—May 1.

Richard Jones Tomlinson, of Bristol, merchant, for an improved rafter for roofs, or beam, for other purposes.—May 3.

John Redhead, of Heworth, Durham, engineer and mariner; and William Parrey, of East-lane, Walworth, master mariner, for certain improvements in propelling vessels.—May 5.

ARTICLE XVII.

METEOROLOGICAL TABLE.

1821.	Wind.	BAROMETER.		THERMOMETER.		Evap.	Rain.	Hygr. at 9 a.m.	
		Max.	Min.	Max.	Min.				
5th Mon.									
May 1	N E	30·05	29·92	61	38	—		65	●
2	N E	29·92	29·82	67	52	—		62	
3	S W	29·82	29·74	71	44	—		67	
4	S W	29·74	29·68	73	42	—		64	
5	S E	29·68	29·50	72	45	52		59	
6	S W	29·87	29 53	62	42	—	04	58	
7	S W	29·96	29·87	65	42	—	02	58	
8	S W	30·16	29·96	63	38	—	07	80	☾
9	N W	30·26	30·16	65	42	50		64	
10	N W	30·25	30·07	64	44	—	—	60	
11	N W	30·07	29·91	70	54	—		73	
12	N W	29·91	29·45	62	41	—		63	
13	N W	29·45	29·44	56	38	45	12	61	
14	N W	29·44	29·29	59	42	—	18	64	
15	S W	29·74	29·29	54	38	—	28	80	
16	W	30·01	29·74	57	38	—	07	65	
17	Var.	30·01	29·92	56	42	—	62	65	○
18	N W	30·27	29·98	60	36	48		71	
19	N W	30·27	30·23	64	40	—	10	61	
20	N E	30·23	30·17	59	31	—		67	
21	N E	30·17	30·06	61	32	—			
22	N E	30·06	29 87	57	37	—			
23	N E	30·01	29·86	51	30	45	—		
24	N	30·03	30·00	55	29	—			☽
25	S W	30·00	29·84	61	54	—	21		
26	N W	29 95	29·84	52	30	—	—		
27	N W	29·98	29·95	57	39	—	05		
28	N W	30·15	29·98	58	38	45	08		
29	N W	30·25	30·15	61	34	—			
30	E	30·26	30·20	66	39	—			
31	E	30·20	30·09	64	40	35			●
		30·27	29·29	73	29	3·20	1·84	58—80	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

REMARKS.

Fifth Month.—1. Cloudy: fine towards evening. 2. Cloudy. 3, 4. Fine. 5. Cloudy: fine. 6. Showery. 7. Fine. 8. Showery. 9. Fine. 10. Cloudy: fine. 11, 12. Fine. 13. Showers. 14. Fine. 15. Showery morning: thunder, p. m. with large hail. 16. Showers. 17. Rainy. 18, 19. Fine. 20. Fine: cold wind. 21. Ditto. 22. Cloudy: cold wind. 23. Showers: cold wind. 24. Cloudy and cold. 25. Cloudy: rainy night. 26. Slight showers: some snow. 27. Cloudy: cold wind. 28. Showery: a thunder-storm in the neighbourhood about five, p. m. with large hail and heavy rain. 29. Fine. 30. Fine. 31. Fine.

RESULTS.

Winds: N, 1; NE, 6; E, 2; SE, 1; SW, 7; W, 1; NW, 12; Var. 1.

Barometer: Mean height	
For the month	29·930 inches.
For the lunar period, ending the 24th	29·907
For 13 days, ending the 11th (moon north)	29·922
For 14 days, ending the 25th (moon south)	29·883
Thermometer: Mean height	
For the month	50·225°
For the lunar period	51·620
For 30 days, the sun in Taurus	53·290
Evaporation	3·20 in.
Rain	1·84
Mean of hygrometer for 20 days	65°

Laboratory, Stratford, Sixth Month, 25, 1821. R. HOWARD.

ANNALS

OF

PHILOSOPHY.

AUGUST, 1821.

ARTICLE I.

On the Magnetic Phenomena produced by Electricity. In a Letter from Sir H. Davy, Bart. FRS. to W. H. Wollaston, MD. PRS.*

MY DEAR SIR,

THE similarity of the laws of electrical and magnetic attraction has often impressed philosophers; and many years ago, in the progress of the discoveries made with the voltaic pile, some inquirers (particularly M. Ritter,†) attempted to establish the existence of an identity or intimate relation between these two powers; but their views being generally obscure, or their experiments inaccurate, they were neglected: the chemical and electrical phenomena exhibited by the wonderful combination of Volta, at that time almost entirely absorbed the attention of scientific men; and the discovery of the fact of the true connexion between electricity and magnetism, seems to have been reserved for M. Oersted, and for the present year.

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* From the Philosophical Transactions, 1821.

† M. Ritter asserted that a needle composed of silver and zinc arranged itself in the magnetic meridian, and was slightly attracted and repelled by the poles of a magnet; and that a metallic wire, after being exposed in the voltaic circuit, took a direction NE and SE. His ideas are so obscure that it is often difficult to understand them; but he seems to have had some vague notion that electrical combinations, when not exhibiting their electrical tension, were in a magnetic state, and that there was a kind of electro-magnetic meridian depending upon the electricity of the earth. (See Annales de Chimie, tom. 64, p. 80.) Since this letter has been written, Dr. Marcet has been so good as to send me from Genoa some pages of Aldini on Galvanism, and of Izarn's Manual of Galvanism, published at Paris more than 16 years ago. M. Mojon, senior, of Genoa, is quoted in these pages as having rendered a steel needle magnetic, by placing it in a voltaic circuit for a great length of time. This, however, seems to have been dependent merely upon its place in the magnetic meridian, or upon an accidental curvature of it; but M. Romagnesi, of Trente, is stated to have discovered that the pile of Volta caused a declination of the needle; the details are not given, but if the general statement be correct, the author could not have observed the same fact as M. Oersted, but merely supposed that the needle had its magnetic poles altered after being placed in the voltaic circuit as a part of the electrical combination.

This discovery, from its importance and unexpected nature, cannot fail to awaken a strong interest in the scientific world; and it opens a new field of inquiry, into which many experimenters will undoubtedly enter: and where there are so many objects of research obvious, it is scarcely possible that similar facts should not be observed by different persons. The progress of science is, however, always promoted by a speedy publication of experiments; hence, though it is probable that the phenomena which I have observed may have been discovered before, or at the same time in other parts of Europe, yet I shall not hesitate to communicate them to you, and through you to the Royal Society.

I found, in repeating the experiments of M. Oersted with a voltaic apparatus of one hundred pair of plates of four inches, that the south pole of a common magnetic needle (suspended in the usual way) placed under the communicating wire of platinum (the positive end of the apparatus being on the right hand), was strongly attracted by the wire, and remained in contact with it, so as entirely to alter the direction of the needle, and to overcome the magnetism of the earth. This I could only explain by supposing that the wire itself became magnetic during the passage of the electricity through it, and direct experiments, which I immediately made, proved that this was the case. I threw some iron filings on a paper, and brought them near the communicating wire, when immediately they were attracted by the wire, and adhered to it in considerable quantities, forming a mass round it 10 or 12 times the thickness of the wire: on breaking the communication, they instantly fell off, proving that the magnetic effect depended entirely on the passage of the electricity through the wire. I tried the same experiment on different parts of the wire, which was seven or eight feet in length, and about the twentieth of an inch in diameter, and I found that the iron filings were every where attracted by it; and making the communication with wires between different parts of the battery, I found that iron filings were attracted, and the magnetic needle affected in every part of the circuit.

It was easy to imagine that such magnetic effects could not be exhibited by the electrified wire without being capable of permanent communication to steel. I fastened several steel needles, in different directions, by fine silver wire to a wire of the same metal, of about the thirtieth of an inch in thickness and 11 inches long, some parallel, others transverse, above and below in different directions; and I placed them in the electrical circuit of a battery of 30 pairs of plates, of nine inches by five, and tried their magnetism by means of iron filings: they were all magnetic: those which were parallel to the wire attracted filings in the same way as the wire itself, but those in transverse directions exhibited each two poles, which, being examined by the test of delicate magnets, it was found that all the needles that were placed under the wire (the positive end of the battery being east) had their north poles on the south side of the wire, and their south poles on the north side; and that those placed over had their south poles turned to the south, and their north poles turned to the north; and this was the case whatever was the inclination of the needles to the horizon. On breaking the connexion, all the steel needles that were on the wire in a transverse direction retained their magnetism, which was as powerful as ever, while those which were parallel to the silver wire appeared to lose it at the same time as the wire itself.

I attached small longitudinal portions of wires of platinum, silver, tin, iron, and steel, in transverse directions, to a wire of platinum that was placed in the circuit of the same battery. The steel and the iron wire immediately acquired poles in the same manner as in the last experiment; the other wires seemed to have no effect, except in acting merely as parts of the electrical circuit; the steel retained its magnetism as powerfully after the circuit was broken as before; the iron wire immediately lost a part of its polarity, and in a very short time the whole of it.

The battery was placed in different directions as to the poles of the earth; but the effect was uniformly the same. All needles placed transversely under the communicating wires, the positive end being on the right hand, had their north poles turned towards the face of the operator, and those above the wire their south poles; and on turning the wire round to the other side of the battery, it being in a longitudinal direction, and marking the side of the wire, the same side was always found to possess the same magnetism; so that in all arrangements of needles transversely round the wire, all the needles above had north and south poles opposite to those below, and those arranged vertically on one side, opposite to those arranged vertically on the other side.

I found that contact of the steel needles was not necessary, and that the effect was produced instantaneously by the mere juxta-position of the needle in a transverse direction, and that through very thick plates of glass: and a needle that had been placed in a transverse direction to the wire merely for an instant, was found as powerful a magnet as one that had been long in communication with it.

I placed some silver wire of 1-20th of an inch and some of 1-50th in different parts of the voltaic circuit when it was completed, and shook some steel filings on a glass plate above them:

the steel filings arranged themselves in right lines always at right angles to the axis of the wire; the effect was observed, though feebly, at the distance of a quarter of an inch above the thin wire, and the arrangement in lines was nearly to the same length on each side of the wire.

I ascertained by several experiments, that the effect was proportional to the quantity of electricity passing through a given space, without any relation to the metal transmitting it: thus, the finer the wires the stronger their magnetism.

A zinc plate of a foot long, and six inches wide, arranged with a copper plate on each side, was connected by a very fine wire of platinum, according to your method; and the plates were plunged an inch deep in diluted nitric acid. The wire did not sensibly attract fine steel filings. When they were plunged two inches, the effect was sensible; and it increased with the quantity of immersion. Two arrangements of this kind acted more powerfully than one; but when the two were combined so as to make the zinc and copper-plates but parts of one combination, the effect was very much greater. This was shown still more distinctly in the following experiment: Sixty zinc plates with double copper-plates were arranged in alternate order, and the quantity of iron filings which a wire of a determinate thickness took up observed: the wire remaining the same, they were arranged so as to make a series of thirty; the magnetic effect appeared more than twice as great; that is, the wire raised more than double the quantity of iron filings.

The magnetism produced by voltaic electricity seems (the wire transmitting it remaining the same) exactly in the same ratio as the heat; and however great the heat of a wire, its magnetic powers were not impaired. This was distinctly shown in transmitting the electricity of 12 batteries of 10 plates each of zinc, with double copper arranged as three, through fine platinum wire, which, when so intensely ignited as to be near the point of fusion, exhibited the strongest magnetic effects, and attracted large quantities of iron filings and even small steel needles from a considerable distance.

As the discharge of a considerable quantity of electricity through a wire seemed necessary to produce magnetism, it appeared probable, that a wire electrified by the common machine would not occasion a sensible effect; and this I found was the case, on placing very small needles across a fine wire connected with a prime conductor of a powerful machine and the earth. But as a momentary exposure in a powerful electrical circuit was sufficient to give permanent polarity to steel, it appeared equally obvious, that needles placed transversely to a wire at the time that the electricity of a common Leyden battery was discharged through it, ought to become magnetic; and this I found was actually the case, and according to precisely the same laws as in the voltaic circuit; the needle *under* the wire, the positive conductor being on the right hand, offering its north pole to the face of the operator, and the needle *above*, exhibiting the opposite polarity.

So powerful was the magnetism produced by the discharge of an electrical battery of 17 square feet highly charged, through a silver wire of 1-20th of an inch, that it rendered bars of steel of two inches long, and from 1-20th to 1-10th in thickness, so magnetic, as to enable them to attract small pieces of steel wire or needles; and the effect was communicated to a distance of five inches above or below or laterally from the wire, through water or thick plates of glass or metal electrically insulated.

The facility with which experiments were made with the common Leyden battery, enabled me to ascertain several circumstances which were easy to imagine, such as that a tube filled with sulphuric acid of one-fourth of an inch in diameter, did not transmit sufficient electricity to render steel magnetic; that a needle placed transverse to the explosion through air, was less magnetized than when the electricity was passed through wire; that steel bars exhibited no polarity (at least at their extremities) when the discharge was made through them as part of the circuit, or when they were placed parallel to the discharging wire; that two bars of steel fastened together, and having the discharging wire placed through their common centre of gravity, showed little or no signs of magnetism after the discharge till they were separated, when they exhibited their north and south poles opposite to each other, according to the law of position.

These experiments distinctly showed, that magnetism was produced whenever concentrated electricity passed through space; but the precise circumstances, or law of its production, were not obvious from them. When a magnet is made to act on steel filings, these filings arrange themselves in curves round the poles, but diverge in right lines; and in their adherence to each other form right lines, appearing as spicula. In the attraction of the filings round the wire in the voltaic circuit, on the contrary, they form one coherent mass, which would probably be perfectly cylindrical were it not for the influence of gravity. In first considering the subject, it appeared to me that there must be as many double poles as there could be imagined points of contact round the wire; but when I found the north and south poles of a needle uniformly attracted by the same quarters of the wire, it appeared to me that there must be four principal poles corresponding to these four quarters. You, however, pointed out to me that there was nothing definite in the poles, and mentioned your idea, that the phenomena might be explained, by supposing a kind of revolution of magnetism round the axis of the wire, depending for its direction upon the position of the negative and positive sides of the electrical apparatus.

To gain some light upon this matter, and to ascertain correctly the relations of the north and south poles of steel magnetized by electricity to the positive and negative state, I placed short steel needles round a circle made on pasteboard, of about two inches and half in diameter, bringing them near each other, though not in contact, and fastening them to the pasteboard by thread, so that they formed the sides of a hexagon inscribed within the circle. A wire was fixed in the centre of this circle, so that the circle was parallel to the horizon, and an electric shock was passed through the wire, its upper part being connected with the positive side of a battery, and its lower part with the negative. After the shock all the wires were found magnetic, and each had two poles; the south pole being opposite to the north pole of the wire next to it, and *vice versâ;* and when the north pole of a needle was touched with a wire, and that wire moved round the circle to the south pole of the same needle, its motion was opposite to that of the apparent motion of the sun.

A similar experiment was tried with six needles arranged in the same manner; with only this difference, that the wire positively electrified was below. In this case the results were precisely the same, except that the poles were reversed; and any body, moved in the circle from the north to the south pole of the same needle, had its direction from east to west.

A number of needles were arranged as polygons in different circles round the same piece of pasteboard, and made magnetic by electricity; and it was found that in all of them, whatever was the direction of the pasteboard, whether horizontal or perpendicular, or inclined to the horizon, and whatever was the direction of the wire with respect to the magnetic meridian, the same law prevailed; for instance, when the positive wire was east, and a body was moved round the circle from the north to the south poles of the same wire; its motion (beginning with the lower part of the circle) was from north to south, or with the upper part from south to north; and when the needles were arranged round a cylinder of pasteboard so as to cross the wire, and a pencil mark drawn in the direction of the poles, it formed a spiral.

It was perfectly evident from these experiments, that as many polar arrangements may be formed as chords can be drawn in circles surrounding the wire; and so far these phenomena agree with your idea of revolving magnetism; but I shall quit this subject, which I hope you will yourself elucidate for the information of the Society, to mention some other circumstances and facts belonging to the inquiry.

Supposing powerful electricity to be passed through two, three, four, or more wires, forming part of the same circuit parallel to each other in the same plane, or in different planes, it could hardly be doubted that each wire, and the space around it, would become magnetic in the same manner as a single wire, though in a less degree; and this I found was actually the case. When four wires of fine platinum were made to complete a powerful voltaic circuit, each wire exhibited its magnetism in the same manner, and steel filings on the sides of the wires opposite attracted each other.

As the filings on the opposite sides of the wire attracted each other in consequence of their being in opposite magnetic states, it was evident, that if the similar sides could be brought in contact, steel filings upon them would repel each other. This was very easily tried with two voltaic batteries arranged parallel to each other, so that the positive end of one was opposite to the negative end of the other: steel filings upon two wires of platinum joining the extremities strongly repelled each other. When the batteries were arranged in the *same* order, *i. e.* positive opposite to positive, they attracted each other; and wires of platinum (without filings) and fine steel wire (still more strongly) exhibited similar phenomena of attraction and repulsion under the same circumstances.

As bodies magnetized by electricity put a needle in motion, it was natural to infer that a magnet would put bodies magnetized by electricity in motion; and this I found was the case. Some pieces of wire of platinum, silver, and copper, were placed separately upon two knife edges of platinum connected with two ends of a powerful voltaic battery, and a magnet presented to them; they were all made to roll along the knife edges, being attracted when the north pole of the magnet was presented, the positive side of the battery being on the right hand, and repelled when it was on the left hand, and *vice versâ*, changing the pole of the magnet. Some folds of gold leaf were placed across the same apparatus, and the north pole of a powerful magnet held opposite to them; the folds approached the magnet, but did not adhere to it. On the south pole being presented, they receded from it.

I will not indulge myself by entering far into the theoretical part of this subject; but a number of curious speculations cannot fail to present themselves to every philosophical mind, in consequence of the facts developed; such as whether the magnetism of the earth may not be owing to its electricity, and the variation of the needle to the alterations in the electrical currents of the earth in consequence of its motions, internal chemical changes, or its relations to solar heat; and whether the luminous effects of the auroras at the poles are not shown, by these new facts, to depend on electricity. This is evident, that if strong electrical currents be supposed to follow the apparent course of the sun, the magnetism of the earth ought to be such as it is found to be.

But I will quit conjectures, to point out a simple mode of making powerful magnets, namely, by fixing bars of steel across, or circular pieces of steel fitted for making horse-shoe magnets, round the electrical conductors of buildings in elevated and exposed situations.*

The experiments detailed in these pages were made with the apparatus belonging to the Royal and London Institution; and I was assisted in many of them by Mr. Pepys, Mr. Allen, and Mr. Stodart, and in all of them by Mr. Faraday.†

I am, my dear Sir, very sincerely yours,

Lower Grosvenor-street, Nov. 12, 1820. Humphry Davy.

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* There are many facts recorded in the Philosophical Transactions which prove the magnetizing powers of lightning; one in particular, where a stroke of lightning passing through a box of knives, rendered most of them powerful magnets.—(See Phil. Trans. No. 157, p. 520; and No. 437, p. 57.)

† All the experiments detailed in this paper, except those mentioned p. 86, were made in the course of October, 1820; the last arose in consequence of a conversation with Dr. Wollaston, and were made in the beginning of November. I find, by the Annales de Chimie et de Physique, for September, which arrived in London Nov. 24, that M. Arago has anticipated me in the discovery of the attractive and magnetizing powers of the wires in the voltaic circuit; but the phenomena presented by the action of common electricity (which I believe as yet have been observed by no other person), induce me still to submit my paper to the Council of the Royal Society. Before any notice arrived of the researches of the French philosophers, I had tried, with Messrs. Allen and Pepys, an experiment, which M. Arago likewise thought of,—whether the arc of flame of the voltaic battery would be affected by the magnet; but from the imperfection of our apparatus, the results were not decisive. I hope soon to be able to repeat it under new circumstances.

I have made various experiments, with the hope of affecting electrified wires by the magnetism of the earth, and of producing chemical changes by magnetism; but without any successful results.

Since I have perused M. Ampere's elaborate treatise on the electro-magnetic phenomena, I have passed the electrical shock along a spiral wire twisted round a glass tube containing a bar of steel, and I found that the bar was rendered powerfully magnetic by the process.

Without meaning to offer any decided opinion on that gentleman's ingenious views, I shall beg permission to mention two circumstances, which seem to me unfavourable to the idea of the identity of electricity and magnetism; first, the great distance to which magnetism is communicated by common electricity (I found that a steel bar was made magnetic at 14 inches distance from a wire transmitting an electric shock from about 70 feet of charged surface); and, secondly, that the effect of magnetizing at a distance by electricity takes place with the same readiness through air and water, glass, mica, or metals; i. e. through conductors and non-conductors.

Article II.

Tables of Temperature, and a Mathematical Development of the Causes and Laws of the Phænomena which have been adduced in Support of the Hypotheses of "Calorific Capacity, Latent Heat," &c. By John Herapath, Esq.

Concluded from p. 56.

TABLE III. (*continued.*)

Observations.	True temp.	Elast. or volume of gas.	Temp. Fahr. +	
Mixture 3 parts tin, 8 bismuth, and 5 lead, melts ······	1171	1371·241	210·2	
Rain water boils, barometer 30 in.	1172·6	1375·000	12·0	
	3	5·929	12·5	
	4	8·276	13·6	
	5	1380·625	14·7	
	6	2·976	15·8	11·28
	7	5·329	17·0	
Saturated solution of salt boils ······	8	7·684	18·1	
	9	1390·041	19·2	
	1180	2·400	220·35	
	1	4·761	1·5	
	2	7·124	2·6	
	3	9·489	3·8	
	4	1401·856	4·9	
Sulphur melts ······	5	4·225	6·0	
	6	6·596	7·2	11·37
	7	8·969	8·3	
	8	1411·344	9·5	
	9	3·721	230·6	
	1190	6·100	1·72	
	1	8·481	2·9	
	2	1420·864	4·0	
	3	3·249	5·2	
	4	5·636	6·3	
	5	8·025	7·5	
	6	1430·416	8·6	11·48
	7	2·809	9·8	
	8	5·204	240·9	
Nitrous acid boils ··	9	7·601	2·1	
	1200	1440·000	3·20	
	1	2·401	4·4	
	2	4·804	5·5	
	3	7·209	6·7	
	4	9·616	7·8	11·57
	5	1452·025	9·0	
	6	4·436	250·2	
	7	6·849	1·3	
	8	9·264	2·5	
	9	1461·681	3·6	
	1210	4·100	4·77	
	11	6·521	5·9	
	12	8·944	7·1	
	13	1471·369	8·3	
	14	3·796	9·4	11·66
	15	6·225	260·6	

Observations.	True temp.	Elast. or volume of gas.	Temp. Fahr. +	
	1216	1478·656	261·8	
	17	1481·089	3·0	
	18	3·524	4·1	
	19	5·961	5·3	
	1220	8·400	6·43	
	1	1490·841	7·6	
	2	3·284	8·8	
	3	5·729	270·0	
	4	8·176	1·1	11·76
	5	1500·625	2·3	
	6	3·076	3·5	
	7	5·529	4·7	
	8	7·984	5·9	
	9	1510·441	7·0	
	1230	2·900	8·19	
	1	5·361	9·4	
	2	7·824	280·6	
	3	1520·289	1·8	
Equal parts of tin and bismuth melts	4	2·756	2·9	
	5	5·225	4·1	
	6	7·696	5·3	
	7	1530·169	6·5	11·86
	8	2·644	7·7	
	9	5·121	8·9	
	1240	7·600	290·05	
	1	1540·081	1·2	
	2	2·564	2·4	
	3	5·049	3·6	
	4	7·536	4·8	
	5	1550·025	6·0	
	6	2·516	7·2	11·95
	7	5·009	8·4	
	8	7·504	9·6	
	9	1560·001	300·8	
	1250	2·500	2·00	
Sulphur burns slowly············	1	5·001	3·2	
	2	7·504	4·4	
	3	1570·009	5·6	
	4	2·516	6·8	
	5	5·025	8·0	12·05
	6	7·536	9·2	
	7	1580·049	310·4	
	8	2·564	1·6	
	9	5·081	2·8	
	1260	7·600	4·05	
	1	1590·121	5·3	

Observations.	True temp.	Elast. or volume of gas.	Temp. Fahr. +		Observations.	True temp.	Elast. or volume of gas.	Temp. Fahr. +	
	1262	1592·644	6·5			1321	1745·041	389·6	
	3	5·169	7·7	12·14		2	7·684	390·9	
	4	7·696	8·9			3	1750·329	2·2	
	5	1600·225	320·1			4	2·976	3·4	12·72
	6	2·756	1·3			5	5·625	4·7	
	7	5·289	2·5			6	8·276	6·0	
	8	7·824	3·7			7	1760·929	7·2	
	9	1610·361	4·9			8	3·584	8·5	
	1270	2·900	326·19			9	6·241	9·8	
	1	5·441	7·4			1330	8·900	401·07	
	2	7·984	8·6			1	1771·561	2·3	
	3	1620·529	9·8			2	4·224	3·6	
	4	3·076	331·1	12·24		3	6·889	4·9	
	5	5·625	2·3			4	9·556	6·2	12·82
	6	8·176	3·5			5	1782·225	7·5	
	7	1630·729	4·7			6	4·896	8·7	
	8	3·284	5·9			7	7·569	410·0	
	9	5·841	7·2			8	1790·244	11·3	
	1280	8·400	338·43			9	2·921	12·6	
	1	1640·961	9·7			1340	5·600	13·89	
	2	3·524	340·9			1	8·281	15·2	
	3	6·089	2·1			2	1800·964	16·5	
	4	8·656	3·3	12·34		3	3·649	17·8	
	5	1651·225	4·6			4	6·336	19·0	12·91
	6	3·796	5·8			5	9·025	420·3	
	7	6·369	7·0			6	1811·716	1·6	
	8	8·944	8·3			7	4·409	2·9	
	9	1661·521	9·5			8	7·104	4·2	
	1290	4·100	350·77			9	9·801	5·5	
	1	6·681	2·0			1350	1822·500	6·80	
	2	9·264	3·2			1	5·201	8·1	
	3	1671·849	4·5			2	7·904	9·4	
	4	4·436	5·7			3	1830·609	430·7	
	5	7·025	7·0	12·43		4	3·316	2·0	
	6	9·616	8·2			5	6·025	3·3	13·01
	7	1682·209	9·4			6	8·736	4·6	
	8	4·804	360·7			7	1841·449	5·9	
	9	7·401	1·9			8	4·164	7·2	
	1300	1690·000	363·20			9	6·881	8·5	
	1	2·601	4·4			1360	9·600	9·81	
	2	5·204	5·7			1	1852·321	441·1	
	3	7·809	6·9		Tin melts	2	5·044	2·4	
	4	1700·416	8·2			3	7·769	3·7	
	5	3·025	9·4	12·53		4	1860·496	5·0	13·10
	6	5·636	370·7			5	3·225	6·4	
	7	8·249	1·9			6	5·956	7·7	
	8	1710·864	2·2			7	8·689	9·0	
	9	3·481	3·4			8	1871·424	450·3	
	1310	6·100	375·73			9	4·161	1·6	
	11	8·721	7·0			1370	6·900	2·91	
	12	1721·344	8·2			1	9·641	4·2	
	13	3·969	9·5			2	1882·384	5·5	
	14	6·596	380·8			3	5·129	6·9	
	15	9·225	2·0	12·62		4	7·876	8·2	13·20
	16	1731·856	3·3			5	1890·625	9·5	
	17	4·489	4·5			6	3·376	460·8	
	18	7·124	5·8			7	6·129	2·1	
	19	9·761	7·1			8	8·884	3·5	
	1320	1742·400	388·35			9	1901·641	4·8	

Observations.	True temp.	Elast. or volume of gas.	Temp. Fahr. +	
	1380	1904·400	466·11	
	1	7·161	7·4	
	2	9·924	8·8	
	3	1912·689	470·1	
	4	5·456	1·4	
	5	8·225	2·8	
	6	1920·996	4·1	13·30
Bismuth melts	7	3·769	5·4	
	8	6·544	6·7	
	9	9·321	8·1	
	1390	1932·100	9·41	
	1	4·881	480·7	
	2	7·664	2·1	
	3	1940·449	3·4	
	4	3·236	4·8	
	5	6·025	6·1	
	6	8·816	7·4	13·39
	7	1951·609	8·8	
	8	4·404	490·1	
	9	7·201	1·5	
	1400	1960·000	2·80	
	1	2·801	4·1	
	2	5·604	5·5	
	3	8·409	6·8	
	4	1971·216	8·2	
	5	4·025	9·5	13·49
	6	6·836	500·9	
	7	9·649	2·	
	8	1982·464	3·6	
	9	5·281	4·9	
	1410	8·400	6·29	
	11	1990·921	7·6	
	12	3·744	9·0	
	13	6·569	510·4	
	14	9·396	11·7	
	15	2002·225	13·1	13·58
	16	5·056	14·4	
	17	7·889	15·8	
	18	2010·724	17·2	
	19	13·561	18·5	
	1420	16·400	19·87	
	1	19·241	521·2	
	2	2022·084	2·6	
	3	4·929	4·0	
	4	7·776	5·3	
	5	2030·625	6·7	13·68
	6	3·476	8·1	
	7	6·329	9·5	
	8	9·184	530·8	
	9	2042·041	2·2	
	1430	4·900	3·55	
	1	7·761	4·9	
	2	2050·624	6·3	
	3	3·489	7·7	
	4	6·356	9·1	
	5	9·225	540·4	13·78
	6	2062·096	1·8	
	7	4·969	3·2	
	8	7·844	4·6	
	1439	2070·721	546·0	
	1440	3·600	7·33	
	1	6·481	8·7	
	2	9·364	550·1	
	3	2082·249	1·5	
	4	5·136	2·9	
Phosphorus boils ..	5	8·025	4·3	13·87
	6	2090·916	5·7	
	7	3·809	7·1	
	8	6·704	8·4	
Oil of turpentine boils	9	9·601	9·8	
	1450	2102·500	561·20	
	1	5·401	2·6	
	2	8·304	4·0	
	3	11·209	5·4	
	4	14·116	6·8	
	5	17·025	8·2	13·97
	6	19·936	9·6	
	7	2122·849	571·0	
	8	5·764	2·4	
	9	8·681	3·8	
	1460	2131·600	5·17	
	1	4·521	6·6	
	2	7·444	8·0	
	3	2140·369	9·4	
	4	3·296	580·8	
	5	6·225	2·2	14·06
	6	9·156	3·6	
	7	2152·089	5·0	
	8	5·024	6·4	
	9	7·961	7·9	
	1470	2160·900	9·23	
Sulphuric acid boils	1	3·841	590·6	
	2	6·784	2·1	
Lead melts	3	9·729	3·5	
	4	2172·676	4·9	14·16
	5	5·625	6·3	
	6	8·576	7·7	
	7	2181·529	9·2	
Linseed oil boils....	8	4·484	600·6	
	9	7·441	2·0	
	1480	2190·400	3·39	
	1	3·361	4·8	
	2	6·324	6·2	
	3	9·289	7·7	
	4	2202·256	9·1	
	5	5·225	610·5	14·26
	6	8·196	12·0	
	7	2211·169	13·4	
	8	14·144	14·8	
	9	17·121	16·3	
	1490	2220·100	17·65	
	1	3·081	19·1	
	2	6·064	620·5	
	3	9·049	2·0	
	4	2232·036	3·4	
	5	5·025	4·8	14·35
	6	8·016	6·3	

Observations.	True temp.	Elast. or volume of gas.	Temp. Fahr. +	
	1497	2241·009	627·7	
	8	4·004	9·2	
	9	7·001	630·6	
	1500	2250·000	2·00	
	1	3·001	3·4	
	2	6 004	4·9	
	3	9·009	6·3	14·45
	4	2262·016	7·8	
	5	5·025	9·2	
	6	8·036	640·6	
	7	2271·049	2·1	
	8	4·064	3·5	
	9	7·081	5·0	
	1510	2280·100	6·45	
	11	3·121	7·9	
	12	6·144	9·3	
	13	9·169	650·8	
	14	2292·196	2·2	
	15	5·225	3·7	14·54
	16	8·256	5·1	
	17	2301·289	6·6	
	18	4·324	8·1	
	19	7·361	9·5	
	1520	10·400	660·99	
	1	13·441	2·4	
	2	16·484	3·9	
	3	19·529	5·4	
	4	2322·576	6·8	
	5	5·625	8·3	14·64
	6	8·676	9·7	
	7	2331·729	671·2	
	8	4·784	2·7	
	9	7·841	4·1	
	1530	2340·900	5·63	
	1	3·961	7·1	
	2	7·024	8·6	
Mercury boils, Dulong and Petit····	3	2350·089	680 0	
	4	3·156	1·5	14·71
	5	6 225	3·0	
	6	9·296	4·4	
	7	2362·369	5·9	
	8	5·444	7·4	
	9	8·521	8·9	
	1540	2371·600	690·37	
	1	4·681		
	2	7·764		
	3	2380·849		
	4	3·936		
	5	7·025		
	6	2390·116		
	7	3·209		
	8	6·304		
	9	9·401		
Zinc melts ········	1550	2402·500	705·20	
	1	5·601		
	2	8·704		
	3	2411·809		
	4	14·916		

Observations.	True temp.	Elast. or volume of gas.	Temp. Fahr. +
	1555	2418·025	
	6	2421·136	
	7	4·249	
	8	7·364	
	9	2430·481	
	1560	3·600	720·13
	1	6·721	
	2	9·844	
	3	2442·969	
	4	6·096	
	5	9·225	
	6	2452·356	
	7	5·489	
	8	8·624	
	9	2461·761	
	1570	4·900	735·15
	1	8·041	
	2	2471·184	
	3	4·329	
	4	7·476	
	5	2480·625	
	6	3·776	
	7	6·929	
	8	2490·084	
	9	3·241	
	1580	6·400	750·27
	1	9·561	
	2	2502·724	
	3	5·889	
	4	9·056	
	5	12·225	
	6	15·396	
	7	18·569	
	8	2521·744	
	9	4·921	
	1590	8·100	765·49
	1	2531·281	
	2	4·464	
	3	7·649	
	4	2540·836	
	5	4·025	
	6	7·216	
	7	2550·409	
	8	3·604	
	9	6·801	
	1600	2560·000	780·80
	1	3·201	
	2	6·404	
	3	9·609	
	4	2572·816	
	5	6·025	
	6	9·236	
	7	2582·449	
	8	5·664	
	9	8·881	
	1610	2592·100	796·21
	11	5·321	
	12	8·544	
	13	2601·769	

Observations.	True temp.	Elast. or volume of gas.	Temp. Fahr. +	Observations.	True temp.	Elast. or volume of gas.	Temp. Fahr. +
	1614	2604·996			2040	4161·6	
	15	8·225			50	4202·5	
	16	2611·456			60	43·6	
	17	4·689			70	84·9	
	18	7·924			80	4326·4	
	19	2621·161			90	68·1	
	1620	4·400	811·71		2100	4410·0	1667·8
	1	7·641			10	52·1	
	2	2630·884			20	94·4	
	3	4·129			30	4536·9	
	4	7·376			40	79·6	
	5	2640·625			50	4622·5	
	6	3·876			60	65·6	
	7	7·129			70	4708·9	
	8	2650·384			80	52·4	
	9	53·641			90	96·1	
	1630	56·9	827·31		2200	4840·0	1875·2
	40	89·6			10	84·1	
	50	2722·5			20	4928·4	
	60	55·6			30	72·9	
	70	88·9			40	5017·6	
	80	2822·4			50	62·5	
	90	56·1			60	5107·6	
	1700	90·0	939·2		70	52·9	
	10	2924·1			80	98·4	
	20	58·4			90	5244·1	
	30	92·9			2300	90·0	2091·2
	40	3027·6			10	5336·1	
	50	62·5			20	82·4	
	60	97·6			30	5428·9	
	70	3132·9			40	75·6	
	80	68·4			50	5522·5	
	90	3204·1			60	69·6	
	1800	40·0	1107·2		70	5616·9	
	10	76·1			80	64·4	
	20	3312·4			90	5712·1	
	30	48·9			2400	60·0	2316·8
	40	85·6			10	5808·1	
	50	3422·5			20	56·4	
	60	59·6			30	5904·9	
	70	96·9			40	53·6	
	80	3534·4			50	6002·5	
	90	72·1			60	51·6	
	1900	3610·0	1284·8		70	6100·9	
	10	48·1			80	50·4	
	20	86·4			90	6200·1	
	30	3724·9			2500	50·0	2552·0
	40	63·6			10	6300·1	
	50	3802·5			20	50·4	
	60	41·6			30	6400·9	
	70	80·9			40	51·6	
	80	3920·4			50	6502·5	
	90	60·1			60	53·6	
	2000	4000·0	1472·0		70	6604·9	
	10	40·1			80	56·4	
	20	80·4			90	6708·1	
	20	4120·9			2600	60·0	2796·8

Observations.	True temp.	Elast. or volume of gas.	Temp. Fahr. +	Observations.	True temp.	Elast. or volume of gas.	Temp. Fahr. +
	2610	6812·1			2810	7996·1	
	20	64·4			20	52·4	
	30	6916·9			30	8008·9	
	40	69·6			40	65·6	
	50	7022·5			50	8122·5	
	60	75·6			60	79·6	
	70	7128·9			70	8236·9	
	80	82·4			80	94·4	
	90	7236·1			90	8352·1	
	2700	90·0	3051·2		2900	8410·0	3588·8
	10	7344·1			10	68·1	
	30	98·4			20	8526·4	
	30	7452·9			30	84·9	
	40	7507·6			40	8643·6	
	50	62·5			50	8702·5	
	60	7617·6			60	61·6	
	70	72·9			70	8820·9	
	80	7728·4			80	80·4	
	90	84·1			90	8940·1	
	2800	7840·0	3315·2		3000	9000·0	3872·0

Table IV.

A Table showing the relations between the indications of a common mercurial and an air thermometer, corrected for the dilatation of glass, according to the degrees of Fahrenheit, and interpolated from the experiments of MM. Dulong and Petit.

Mercurial thermometer in Fahrenheit.	Air thermometer corrected for glass in Fahrenheit.	First difference, or amount of correction or difference of thermometers. −	Second difference, or difference of differences.
212	212·00	0·00	
22	21·77	0·23	$\cdot 22\dot{8}$
32	31·53	0·47	$\cdot 23\dot{6}$
42	41·29	0·71	$\cdot 24\dot{4}$
52	51·04	0·96	$\cdot 25\dot{2}$
62	60·78	1·22	$\cdot 25\dot{9}$
72	70·51	1·49	$\cdot 26\dot{7}$
82	80·23	1·77	$\cdot 27\dot{5}$
92	89·95	2·05	$\cdot 28\dot{3}$
302	99·66	2·34	$\cdot 29\dot{1}$
12	309·36	2·64	$\cdot 29\dot{8}$
22	19·05	2·95	$\cdot 30\dot{6}$
32	28·74	3·26	$\cdot 31\dot{4}$
42	38·42	3·58	$\cdot 32\dot{2}$
52	48·09	3·91	$\cdot 32\dot{9}$

Mercurial thermometer in Fahrenheit.	Air thermometer corrected for glass in Fahrenheit.	First difference, or amount of correction or difference of thermometers.	Second difference, or difference of differences.
52	48·09	3·91	·33$\dot{7}$
62	57·75	4·25	·34$\dot{5}$
72	67·40	4·60	·35$\dot{3}$
82	77·05	4·95	·36$\dot{1}$
92	86·69	5·31	·36$\dot{8}$
402	96·32	5·68	·37$\dot{6}$
12	405·94	6·06	·38$\dot{4}$
22	15·56	6·44	·39$\dot{2}$
32	25·17	6·83	·39$\dot{9}$
42	34·77	7·23	·40$\dot{7}$
52	44·36	7·64	·41$\dot{5}$
62	53·94	8·06	·42$\dot{3}$
72	63·52	8·48	·43$\dot{1}$
82	73·09	8·91	·43$\dot{8}$
92	82·65	9·35	·44$\dot{6}$
502	92·20	9·80	·45$\dot{4}$
12	501·75	10·25	·46$\dot{2}$
22	11·29	10·71	·46$\dot{9}$
32	20·82	11·18	·47$\dot{7}$
42	30·34	11·66	·48$\dot{5}$
52	39·85	12·15	·49$\dot{3}$
62	49·36	12·64	·50$\dot{1}$
72	58·86	13·14	·50$\dot{8}$
82	68·35	13·65	·51$\dot{6}$
92	77·83	14·17	·52$\dot{4}$
602	87·31	14·69	·53$\dot{2}$
12	96·78	15·22	·53$\dot{9}$
22	606·24	15·76	·54$\dot{7}$
32	15·69	16·31	·55$\dot{5}$
42	25·13	16·87	·56$\dot{3}$
52	34·57	17·43	·57$\dot{1}$
62	44·00	18·00	·57$\dot{8}$
72	53·42	18·58	·58$\dot{6}$
82	62·83	19·17	·59$\dot{4}$
692	672·24	19·76	

This table I computed from the subsequent account of Dulong and Petit's observations, extracted from vol. xiii. of the *Annals of Philosophy*.

Temp. indicated by mercurial thermometer in centigrade.	Temp. indicated by air thermometer corrected for dilatation of glass.
− 36°	− 36·00°
0	0·0
100	100·0
150	148·70
200	197·05
250	245·05
300	292·70
360	300·00

In the 48th volume of the Philosophical Magazine, I find the numbers in the higher ranges are different. I have not this volume at hand; and, therefore, cannot tell whether Dr. Thomson and Mr. Tilloch refer to the same or different experiments of Dulong and Petit. The numbers in the 48th volume of the Philosophical Magazine, as sent me by my cousin Mr. William Herapath, stand thus:

Merc. therm. centigrade.	Air therm. centigrade.
100	100·00
150	148·70
200	197·05
250	244·17
300	291·77

N.B. When interpolating the observations of Dulong and Petit, I perceived the whole of them, except the last, were connected by a law, which, being very simple, and differing not more than a degree of Fahrenheit from their last observation at 680°, the boiling point of mercury, I have thought it better to continue than to render the table discontinuous.

TABLE V.

True Temperature.

The following table, calculated from the preceding, exhibits the corrections necessary to reduce the temperature of the common mercurial thermometer to the air thermometer, supposing them divided, according to the law of temperature, as resulting from our theory of heat.

Common mercurial therm.	Corrections. —	Diff. of correction.	Common mercurial therm.	Corrections. —	Diff. of correction.
1170	00·00		50	5·13	
Water boils 72·6	00·00		60	5·49	·36
80	·17	·17	70	5·86	·37
90	·40	·23	80	6·23	·37
1200	·64	·24	90	6·61	·38
10	·89	·25	1400	7·00	·39
20	1·15	·26	10	7·40	·40
30	1·41	·26	20	7·81	·41
40	1·68	·27	30	8·22	·41
50	1·96	·28	40	8·64	·42
60	2·24	·28	50	9·07	·43
70	2·53	·29	60	9·51	·44
80	2·83	·30	70	9·95	·44
90	3·14	·31	80	10·40	·45
1300	3·45	·31	90	10·86	·46
10	3·77	·32	1500	11·33	·47
20	4·10	·33	10	11·81	·48
30	4·44	·34	20	12·30	·49
40	4·78	·34	30	12·79	·49
50	5·13	·35	40	13·29	·50

N. B. This table, though calculated to hundredths, can hardly be depended on to agree with the preceding nearer than to tenths, which are as near, perhaps, as we can generally depend on our observations in those high temperatures.

The introduction and titles sufficiently explain the nature and use of the preceding tables; I shall, therefore, merely illustrate their utility by such examples as offer themselves in the course of the following inquiries.

In p. 403, vol. i. New Series of the *Annals*, I have given some formulæ for the mixture of equal and unequal portions of the same fluid; and in pp. 406 and 407, I have also developed my ideas of the causes of what are called "calorific capacity, latent heat, &c." With this simple exposition, it is more than probable I should have rested satisfied, had not my friend the Rev. H. S. Trimmer, struck with the comprehensive simplicity of my views, strongly urged me to turn my attention again to the subject, and to develop my theory a little more in detail. To this gentleman, therefore, and the kind assistance he has promised me at a proper season in the prosecution of the necessary experiments, are the present inquiries chiefly to be attributed.

My object in this paper is to demonstrate, in the usual way of mathematicians, the laws of the phænomena connected with the hypotheses of "capacity for caloric, latent heat,

&c." in order to give philosophers an opportunity of examining the truth of my views on an extensive scale. I shall also myself, as I proceed, compare the formulæ with the best experiments I have at hand, that it may be seen how correctly the theory I have developed agrees with the results of observation. If, however, in the course of this comparison, it should appear necessary to point out any inaccuracies in the methods or experiments of others, philosophers will, I trust, receive my observations with candour, and do me the justice to believe, that though I may differ on certain scientific points from some, I have not the less respect for their labours or their abilities, and have only two objects in view;—the extension of science, and the investigation of truth.

On the Temperatures resulting from the Mixture of Bodies which do not act chemically on one another, and on the Cause of the Phænomena attributed to "Calorific Capacity or Specific Heat."

Prop. I. Theor. I.

If two portions, $W\ W_1$, of the same fluid at the true temperatures $T\ T_1$ be mixed together, the true temperature τ of the mixture, when no extraneous temperature interferes, is equal to $\frac{TW + T_1 W_1}{W + W_1}$.

Because by my theory of heat, the temperature of any body is proportional to the motion, or the intensity of vibration of one of its particles, the sum of all the motions of the particles of any portion of a body, will be as the temperature and the weight of that portion conjointly. And because no extraneous motion or temperature is supposed to interfere, there can be no acquisition or loss of motion in the two parts taken together by the mixture; and, therefore, the sum of all the motions of the two parts will be the same before and after the mixture. But the sum of all the motions before the mixture is as $T\ W + T_1\ W_1$, and the sum of all after the mixture as $(W + W_1)\,\tau$; therefore $(W + W_1)\,\tau = T\ W + T_1\ W_1$ and $\tau = \frac{T\ W + T_1\ W_1}{W + W_1}$.

Cor 1.—If $W = W_1$ $\tau = \frac{T + T_1}{2}$.

Cor. 2.—By Prop. 9 of my preceding paper, the true temperatures are in the subduplicate ratio of the spaces occupied by the same portion of gas under an invariable compression; and these spaces, if the temperatures are measured by the air thermometer, have the same ratio as the corresponding degrees of Fahrenheit + 448. Therefore $T : T_1 :: \sqrt{448 + F} : \sqrt{448 + F_1}$ in which F is the Fahrenheit temperature corresponding to T, and F_1 that corresponding to T_1. In the same way, if F_{11} corresponds to τ, we have $T : \tau :: \sqrt{448 + F} : \sqrt{448 + F_{11}}$

Substituting in the equation of the preceding cor. the values of τ and T_{1} derived from these equations, we obtain $\frac{\sqrt{448 + F_{11}}}{\sqrt{448 + F}}$

$$= \frac{1 + \sqrt{\left(\frac{448 + F_{1}}{448 + F}\right)}}{2}; \text{ whence } F_{11} = (448 + F) \times$$

$$\left\{\frac{1 + \sqrt{\left(\frac{448 + F_{1}}{448 + F}\right)}}{2}\right\}^{2} - 448,$$

the same as I have given in p. 403 of the first volume of the *Annals*, New Series. This theorem, however, as it was then published, I obtained by a very different process from a complex theorem on the mixture of gases.

Cor. 3.—When $n\,W = W_{1}$ $\quad \tau = \frac{T + n\,T_{1}}{n + 1}$.

Cor. 4.—By pursuing the same course, as in the third corollary, we get $\sqrt{\left(\frac{448 + F_{11}}{448 + F}\right)} = \frac{1 + n \times \sqrt{\left(\frac{448 + F_{1}}{448 + F}\right)}}{n + 1}$; whence,

therefore, $F_{11} = (448 + F) \times \left\{\frac{1 + n \times \sqrt{\left(\frac{448 + F_{1}}{448 + F}\right)}}{n + 1}\right\}^{2}$

$- 448$, which is the other theorem I have given in the same p. 403, and which I also deduced from a theorem on the mixture of gases.

Scholium.

These theorems being investigated without any reference to the law of expansion of the body experimented with, should hold good whatever body we use, whether it be a solid, fluid, or air, as long as we can make the experiments correctly with it, and whatever be its relative rate of expansion to other bodies, provided, however, the portions of the body be mixed in the same state, that is, either in the solid, or fluid, or aeriform. Unfortunately, we have no extensive series of experiments to compare these formulæ with. De Luc has made two with water within the temperatures of ice melting and water boiling; and I have made several others, including a much greater range, with mercury.

De Luc mixed water at 200·7° with an equal weight at 45° of Fahrenheit, and found the temperature of the mixture 122·8° — 2·5° = 120·3°.

By the preceding tables,

200·7° give T .... = 1162·6

and 45° give T^{1} .... = 1013·5

2)2176·1

and, therefore, by Cor. 1 τ ...... = 1088·05 which corresponds to 120·2° Fahrenheit. This experiment, therefore, differs from the theory $\frac{1}{10}$th of a degree of Fahrenheit. If Fahrenheit's thermometer really indicated the true increments of temperature, the result should have been the arithmetical mean of 200·7° and 45°, or 122·8°, which is 2·5° more than it was observed.

In another experiment, De Luc mixed equal weights of water at 212° and 32°, and found the result 119°.

212° of Fahr. in the tables, give T = 1172·6
And 32° give T_1 . . . . = 1000

2)2172·6

And consequently τ . . . . = 1086·3

which makes the temperature of the mixture in Fahrenheit = 118·4°. This differs from the experiment ·6 in defect, while the experiment is 3° below the arithmetical mean of 212° and 32°.

Having contrived an apparatus for making the mixture so as not to lose by radiation, or the unequal temperature of the matter of the vessel, I mixed about 30 ounces of mercury at 348·9° with an equal weight at 31°, and found the mixture at 179·3° of Fahrenheit.

Here T . . . . = 1288·5
And T_1 . . . . = 999·0

2)2287·5

Whence τ . . . . = 1143·75

which agrees with 179·9 of Fahrenheit.

This experiment is ·6° below the theory, and 10·7° below the arithmetical mean. The other experiments that I made gave a like coincidence with theory; but as they have lately been read before the Royal Society, it might be deemed improper at present to quote any more of them here.

Were the temperatures 3872° and 32° of Fahrenheit,

Then 3872 would give 3000
And 32 1000

2)4000

2000

which give 1472° for the mean temperature according to Fahrenheit. The arithmetical mean of 3872 and 32 is 1952; that is, 480° above the computation. Wherefore could we make experiments on these extreme ranges, our thermometers would differ no less than 480°, on the supposition of the common theoretical notions of equal increments of heat being accompanied with equal augmentations of volume.

Thus in three experiments made by two different individuals, at different times, with different instruments, widely different fluids, and, perhaps, different methods of making the experiments, the errors of a theory which I had investigated before I knew that any experiments of the kind had been made, or even before I had apparatus, or thought of making experiments of this kind myself, are only — °·1 — °·6 and + °·6; while the errors of the old theory are + 2·5° + 3° and + 10·7°. Philosophers will know how to appreciate the merits of a theory so investigated and so confirmed. Other facts I have no doubt I shall be able to adduce before I get to the end of this paper equally satisfactory and equally convincing of the soundness and the accuracy of the views I have taken.

It must not be concealed, that Dr. Crawford has made some experiments which have been thought to invalidate those of De Luc. "That philosopher exposed a thermometer equally, it is said, to the influence of air heated by watery vapour to 212°, and of air cooled by snow to 32°, and found the thermometer thus exposed stand something lower than 122°, but above what De Luc has stated." Hence Crawford concludes, that the mercurial thermometer, at least between the temperatures of water freezing and water boiling, is a correct indication of the temperature. Probably few men were better qualified, on account of his care and ingenuity, for deciding a question of this importance than Crawford, but certainly no one ever employed in a subject of consequence a more inaccurate and inefficient method than he has in this. Of all bodies, the aeriform are the most improper he could have selected for experiments of this kind. The extreme facility with which they acquire the temperature of any bodies they may come in contact with; the great difficulty with which they communicate the temperature of others; and the almost utter impossibility of appreciating the equality of their influence; conspire to render every thing deduced from such experiments suspicious and equivocal. But in spite of all this, Crawford found that his experiments favoured the general conclusion of De Luc; namely, that the true mean is beneath Fahrenheit's arithmetical. De Luc undoubtedly took the most direct and simple course for settling this point that could be devised; and had he chosen a different fluid, and made his experiments embrace a greater range, he would most probably have discovered the true laws of temperature.

M. Biot, in his Traité de Physique, thinks, that as the coefficient indicating "the specific heat of a body" is constant from the melting of ice to the boiling of water, "we ought to conclude that the degrees of the mercurial thermometer between these limits are proportional to the increments of temperature." In this M. Biot does not consider that he is endeavouring to establish one hypothesis by another, the very probability of which remains to be shown; namely, the hypothesis of "specific heat." Without, however, stopping now to discuss this subject, I will endeavour to show that a very trifling error in MM. Lavoisier and Laplace's experiments with the calorimeter, from which M. Biot drew his conclusion, is sufficient to make these experiments coincide with my theory. By a formula which I shall hereafter demonstrate, if W be a given weight of water, and T its true temperature, the weight of ice it will melt in the calorimeter is $\frac{(T - 1000)\ 22\ W}{3000}$. Therefore, 100 ounces of water at 212° Fahr. will melt 126·57 ounces; and hence 100 ounces of mercury at the same temperature ought to melt only $\frac{29}{1000} \times 126\cdot57 = 3\cdot67$ ounces. Consequently 100 ounces at 118·6°* should melt only 1·835 ounce; and at 122° only a 27th part more, or 1·903 ounce. Thus in operating with 100 ounces of mercury, an error of only $\frac{1}{27}$ of the whole ice melted, or indeed of half of it $\frac{1}{54}$, or of $\frac{34}{1000}$ oz. would be sufficient to overturn the inference of M. Biot. And if Lavoisier and Laplace operated with a less quantity of mercury, 50 ounces for instance, an error of $\frac{1}{54}$, or $\frac{34}{1000}$ oz. would be enough to reduce the law of temperature from the simple to the subduplicate ratio. But if so trifling an error in the quantity of melted ice can produce so enormous a difference in the results, to what a degree of accuracy ought the experiments to be made to establish any law of temperature whatever!

Thus the experiments with the calorimeter are exposed to difficulties and errors against which it is doubtful whether even the abilities of Biot, Lavoisier, and Laplace, could protect them. Mercury, on account of the smallness of its numeratom, or heating power, is certainly one of the worst fluids that could have been chosen for determining the accuracy of the thermometric scale with the calorimeter. Water would be much preferable if the condensation of its vapour could be avoided. This, in experiments with the calorimeter, will always make in favour of the old theory, and against the new. By a formula which I have investigated from my theory, and an experiment related by Dr. Thomson, in his System of Chemistry, I find that one ounce of steam at 212° Fahr. condensed on 100 ounces of water at 201°, would be sufficient to raise it to 212°. Consequently, if a quarter of an ounce of steam, or a quantity equal to 1-400th part of the weight of water, were introduced with the water, it would be enough to render the experiment so doubtful as not be able to determine from it whether the law of temperature followed the simple or the subduplicate ratio. With either fluid, therefore, it appears that the calorimeter is not susceptible of that accuracy which experiments of this kind require. Indeed it is questionable whether there is any method so simple and conclusive as that of mixtures. The only things it appears necessary to guard against are loss of temperature by radiation, and the contact of air of a different temperature from the mixture, and the influence of the temperature of the vessel in which the mixture is made. But both these things seem to be almost effectually avoided by an apparatus similar to the one I have described in my experiments on the Ratio and Law of Temperature lately read before the Royal Society; or at least their effects so much diminished as to be nearly, if not wholly, insensible. Any other way of obviating such losses, as by calculation after the manner of Crawford, experience has convinced me is liable to errors and inaccuracies, which might be a little diminished, but, I think, cannot be avoided.

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* By mistake, I assumed the true mean temperature to correspond with 118·6°, instead of 118·4°; but the difference is so trifling that I think it useless to repeat the calculations, even simple as they are.

Some philosophers suppose that the law of heat I have unfolded is the same as that some time since propounded by Mr. Dalton; there is, however, scarcely the least similarity between them. Mr. Dalton thinks the expansions of all fluids are proportional to the squares of their temperatures from the points of their greatest density, but these points of greatest density are not here taken into account, nor do I even make the rate of the fluid expansions hold any part in my investigations. The theory that I have given, so far as I have yet delivered it, is wholly independent of the law or laws of fluid expansion, and does not even consider whether fluids expand by general or particular laws, nor whether they have points of greatest density or not. Mr. Dalton's theory makes the expansions of all gaseous bodies to follow a geometrical progression, while the temperatures follow an arithmetical; or that the temperatures from some given point are as the logarithms of the expansions; but, according to my theory, the squares of the true temperatures are as the volumes, and, therefore, the first differences of the volumes, or the increments of expansion, are proportional to the second differences of the corresponding temperatures,—a law which differs materially from Mr. Dalton's; for his increments of temperature bear no ratio whatever to any order of the differences of expansion. Notwithstanding, however, this wide difference between the two laws, it must be confessed that Mr. Dalton has approached nearer to the ideas I have developed than any other philosopher I know of; and had he applied his views of fluid expansion to gases, he would have anticipated the general law of temperature I have given.

(To be continued.)

Article III.

On Two New Compounds of Chlorine and Carbon, and on a new Compound of Iodine, Carbon, and Hydrogen. By Mr. Faraday, Chemical Assistant in the Royal Institution.*

One of the first circumstances that induced Sir H. Davy to doubt the compound nature of what was formerly called oxymuriatic acid gas, was the want of action of heated charcoal upon it; and considerable use of the same agent, and of the phenomena exhibited by it in different circumstances with chlorine, was afterwards made in establishing the simple nature of that body.

The true nature of chlorine being ascertained, it became of importance to form all the possible compounds of it with other elementary substances, and to examine them in the new view had of their nature. This investigation has been pursued with such success at different times, that very few elements remain uncombined with it; but with respect to carbon, the very circumstance which first tended to correct the erroneous opinions which, after Scheele's time, and before the year 1810, had gone abroad respecting its nature, proved an obstacle to the formation of its compounds; and up to the present time, the chlorides of carbon have escaped the researches of chemists.

That the difficulty met with in forming a compound of chlorine and carbon was probably not owing to any want or weakness of affinity between the two bodies, was pointed out by Sir H. Davy; who, reasoning on the triple compound of chlorine, carbon, and hydrogen, concluded that the attraction of the two bodies for each other was by no means feeble; and the discovery of phosgene gas by Dr. Davy, in which chlorine and carbon are combined with oxygen, was another circumstance strongly in favour of this opinion.

I was induced last summer to take up this subject, and have been so fortunate as to discover two chlorides of carbon, and a compound of iodine, carbon, and hydrogen, analogous in its nature to the triple compound of chlorine, carbon, and hydrogen, sometimes called chloric ether. I shall endeavour in the following pages to describe these substances, and give the experimental proofs of their nature.

If chlorine and olefiant gas be mixed together, it is well known that condensation takes place, and a colourless limpid volatile fluid is produced, containing chlorine, carbon, and hydrogen.* If the volumes of the two gases are equal, the condensation is perfect. If the olefiant gas is in excess, that excess is left unchanged. But if the chlorine is in excess, the fluid becomes of a yellow tint, and acid fumes are produced. This circumstance alone proves that chlorine can take hydrogen from the fluid; and, on examination, I found it was without the liberation of any carbon or chlorine.

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* From the Philosophical Transactions, 1821.

That the action thus began, might be carried to its utmost extent, some of the pure fluid (chloric ether) was put into a retort with chlorine, and exposed to sunshine. At the first instant of contact between the chlorine and the fluid, the latter became yellow ; but when in the sun's rays, a few moments sufficed to destroy the colour both of the fluid and the chlorine, heat being at the same time evolved. On opening the retort, there was no absorption, but it was found full of muriatic acid gas. This was expelled, and more chlorine introduced, and the whole again exposed to sun light : the colour again disappeared, and a few moist crystals were formed round the edge of the fluid. Chlorine being a third time introduced, and treated as before, it still removed more hydrogen ; and now a sublimate of crystals lined the retort. Proceeding in this way until the chlorine exerted no farther action, the fluid entirely disappeared, and the results were, the dry crystalline substance, and muriatic acid gas.

A portion of olefiant gas was then mixed in a retort with eight or nine times its bulk of chlorine, and exposed to sun light. At first the fluid formed ; but this instantly disappeared; the retort became lined with crystals, and the colour of the chlorine very much diminished.

On examining these crystals, I found they were the compound I was in search of ; but before I give the proofs of their nature, I will describe the process by which this chloride of carbon can be obtained pure.

Perchloride of Carbon.

A glass vessel was made in the form of an alembic head, but without the beak ; the neck was considerably contracted, and had a brass cap with a stop-cock cemented on ; at the top was a small aperture, into which a ground stopper fitted air tight. The capacity of the vessel was about 200 cubic inches. Being exhausted by the air-pump, it was nearly filled with chlorine ; and being then placed over olefiant gas, and as much as could enter having passed in, the stop-cocks were shut, and the whole left for a short time. When the fluid compound of chlorine and olefiant gas had formed and condensed on the sides of the vessel, it was again placed over olefiant gas, and, in consequence of the condensation of a large portion of the gases, a considerable quantity more entered. This was left, as before, to combine with part of the remaining chlorine, to condense, and to form a partial vacuum ; which was again filled with olefiant gas, and the process repeated until all the chlorine had united to form the fluid, and the vessel remained full of olefiant gas. Chlorine was then admitted in repeated portions as before; consequently more of the fluid formed; and ultimately a large portion was obtained in the bottom of the vessel, and an atmosphere of chlorine above it. It was now exposed to sun light. The chlorine immediately disappeared, and the vessel became filled with muriatic acid gas. Having ascertained that water did not interfere with the action of the substances, a small portion was admitted into the vessel which absorbed the muriatic acid gas, and then another atmosphere of chlorine was introduced. Again exposed to the light, this was partly combined with the carbon, and partly converted into muriatic acid gas; which, being as before absorbed by the water, left space for more chlorine. Repeating this action, the fluid gradually became thick and opaque from the formation of crystals in it, which at last adhered to the sides of the glass as it was turned round; and ultimately the vessel only contained chlorine with the accumulated gaseous impurities of the successive portions; a strong solution of muriatic acid, coloured blue from the solution of a little brass, and the solid substance.

I have frequently carried the process thus far in retorts; and it is evident that any conveniently formed glass vessel will answer the purpose. The admission of water during the process prevents the necessity of repeated exhaustion by the air-pump, which cannot be done without injury to the latter; but to have the full advantage of this part of the process, the gases should be as pure as possible, that no atmosphere foreign to the experiment may collect in the vessel.

In order to cleanse the substance, the remaining chlorine and muriatic acid were blown out of the vessel by a pair of bellows, introduced at the stoppered aperture, and the vessel afterwards filled with water, to wash away the muriatic acid and other soluble matters. Considerable care is then requisite in the further purification of the chloride. It retains water, muriatic acid, and a substance, which I find to be a triple compound of chlorine, carbon, and hydrogen, formed from the cement of the cap; and as all these contain hydrogen, a small quantity of any one remaining with the chloride would, in analysis, give erroneous results. Various methods of purification may be devised, founded on the properties of the substance, but I have found the following the most convenient. The substance is to be washed from off the glass, and poured with the water into a jar; a little alcohol will remove the last portions which adhere to the glass; and this, when poured into the water, will precipitate the chloride, and the whole will fall to the bottom of the vessel. Then having decanted the water, the chloride is to be collected on a filter, and dried as much as may be by pressure between folds of bibulous paper. It should next be introduced into a glass tube, and sublimed by a spirit lamp: the pure substance with water will rise at first, but the last portions will be partially decomposed, muriatic acid will be liberated, and charcoal left. The sublimed portion is then to be dissolved in alcohol, and poured into a weak solution of potash, by which the substance is thrown down, and the muriatic acid neutralized and separated; then wash away the potash and muriate by repeated affusions of water, until the substance remains pure; collect it on a filter, and dry it, first between folds of paper, and afterwards by sulphuric acid in the exhausted receiver of the air-pump.

It will now appear as a white pulverulent substance; and if perfectly pure will not, when a little of it is sublimed in a tube, leave the slightest trace of carbon, or liberate any muriatic acid. A small portion of it dissolved in ether, should give no precipitate with nitrate of silver. If it be not quite pure, it must be resublimed, washed, and dried until it is pure.

This substance does not require the direct rays of the sun for its formation. Several tubes were filled with a mixture of one part of olefiant gas with five or six parts of chlorine, and placed over water in the light of a dull day; in two or three hours there was very considerable absorption, and crystals of the substance were deposited on the inside of the tubes. I have also often observed the formation of the crystals in retorts in common day light.

A retort being exhausted had 12 cubic inches of olefiant gas introduced, and 24·75 cubic inches of chlorine: as soon as the condensation occasioned by the formation of the fluid had taken place, 21·5 cubic inches more of chlorine were passed in, and the retort set aside in a dark place for two days. At the end of that time muriatic acid gas and the solid chloride had formed, but the greater part of the fluid remained unchanged. Hence it will form even in the dark by length of time.

I tried to produce the chloride by exposure of the two gases in tubes over water to strong lamp light for two or three hours, but could not succeed.

The perchloride of carbon, when pure, is immediately after fusion, or sublimation, a transparent colourless substance. It has scarcely any taste. Its odour is aromatic, and approaching to that of camphor. Its specific gravity is as nearly as possible 2. Its refractive power is high, being above that of flint glass (1·5767). It is very friable, easily breaking down under pressure; and when scratched has much of the feel and appearance of white sugar. It does not conduct electricity.

The crystals obtained by sublimation and from solutions of the substance in alcohol and ether, are dendritical, prismatic, or in plates; the varieties of form, which are very interesting, are easily ascertained, and result from a primitive octohedron.

It volatilizes slowly at common temperatures, and passes, in the manner of camphor, towards the light. If warmed, it rises more rapidly, and then forms fine crystals: when the temperature is further raised, it fuses at 320° Fahr. and boils at 360°

under atmospheric pressure. When condensed again from these rapid sublimations, it concretes in the upper part of the tube or vessel containing it, in so transparent and colourless a state, that it is difficult, except from its high refractive power, to perceive where it is lodged. As the crust it forms becomes thicker, it splits, and cracks like sublimed camphor; and in a few minutes after it is cold, is white, and nearly opaque. If the heat be raised still higher, as when the substance is passed through a red hot tube, it is decomposed, chlorine is evolved, and another chloride of carbon, which condenses into a fluid, is obtained. This shall be described presently.

It is not readily combustible; when held in the flame of a spirit lamp, it burns with a red flame, emitting much smoke and acid fumes; but when removed from the lamp, combustion ceases. In the combustion that does take place in the lamp, the hydrogen of the alcohol, by combining with the chlorine of the compound, performs the most important part; nevertheless, when the substance is heated red in an atmosphere of pure oxygen, it sometimes burns with a brilliant light.

It is not soluble in water at common temperatures; or only in very small quantity. When a drop or two of the alcoholic solution is poured into a large quantity of water, it renders it turbid from the deposition of the substance. It does not appear that hot water dissolves more of it than cold water.

It dissolves in alcohol with facility, and in much greater quantity with heat than without. A saturated hot solution crystallizes as it cools, and the cold solution also gives crystals by spontaneous evaporation. When poured into water, the chloride is precipitated, and falls to the bottom in flakes. If burnt, the flame of the alcohol is brightened by the presence of the substance, and fumes of muriatic acid are liberated. Solution of nitrate of silver does not produce any turbidness in it, unless it be in such quantity that the water throws down the substance; but no chloride of silver is formed.

It is much more soluble in ether than in alcohol, and more so in hot than in cold ether. The hot solution deposits crystals as it cools; and the crystallization of a cold solution, when evaporated on a glass plate, is very beautiful. This solution is not precipitated by water, unless the ether has previously been dried, and then water occasions a turbidness. Nitrate of silver does not precipitate it. When burned, muriatic acid fumes are liberated, but the greater part of the chloride remains in the capsule.

It is soluble in the volatile oils, and on evaporation is again obtained in crystals. It is also readily soluble in fixed oils. The solutions when heated liberate muriatic acid gas, and the oil becomes of a dark colour, as if charred.

Solutions of the acids and alkalies do not act with any energy on the substance. When boiled with solutions of pure potash and soda, it rises and condenses in the upper part of the vessel;

and though it be brought down to the alkali many times, and reboiled, still the alkali, when examined, is not found to contain any chlorine, nor is any change produced. Ammonia in solution is also without action upon it. These solutions do not appear to dissolve more of it than pure water.

Muriatic acid in solution does not act at all upon it. Strong nitric acid boiled upon it dissolves a portion, but does not decompose it: as it cools, part of the chloride is deposited unaltered, and the concentrated acid, when diluted, lets more fall down. The diluted portion being filtered, and tested with nitrate of silver, gives no precipitate. It does not appear to be either soluble in, or acted upon by, concentrated sulphuric acid. It sinks slowly in the acid, and, when heated, is converted into vapour, which, rising through the acid, condenses in the upper part of the tube.

It is not acted upon by oxygen at temperatures under a red heat. A mixture of oxygen and the vapour of the substance would not inflame by a strong electric spark, though the temperature was raised by a spirit-lamp to about 400°. When oxygen mixed with the vapour of the substance is passed through a red-hot tube, there is decomposition; and mixtures of chlorine, carbonic oxide, carbonic acid, and phosgene gases are produced. A portion of the chloride was heated with peroxide of mercury in a glass tube over mercury; as soon as the oxide had given off oxygen, and the heat had risen so high as to soften the glass considerably, the vapour suddenly detonated with the oxygen with bright inflammation. The substances remaining were oxygen, carbonic acid, and calomel; and I believe there was no decomposition or action, until so much mercury had risen in vapour as to aid the oxygen by a kind of double affinity in decomposing the chloride of carbon.

Chlorine produces no change on the substance, either by exposure to light or heat.

When iodine is heated with it at low temperatures, the two substances melt and unite, and there is no further action. When heated more strongly in vapour, the iodine separates chlorine, reducing the perchloride to the fluid protochloride of carbon, and chloriodine is produced. This dissolves, and if no excess of iodine be present, the whole remains fluid at common temperatures. When water is added, it generally liberates a little iodine; and on heating the solution, so as to drive off all free iodine, and testing by nitrate of silver, chloride and iodide of silver are obtained.

Hydrogen and the vapour of the substance would not inflame at the temperature of 400° Fahr. by strong electrical sparks; but when the mixture was sent through a red-hot tube, the chloride was decomposed, and muriatic acid gas and charcoal produced.

The vapour of the perchloride of carbon readily detonates by the electric spark with a mixture of oxygen and hydrogen gases; but the gaseous results are very mixed and uncertain, from the near equipoise of affinities that exist among the elements.

Sulphur readily unites to it when melted with it, and the mixture crystallizes on cooling into a yellowish mass. When heated more strongly, the substance rises unchanged, and leaves the sulphur unaltered; but when the mixed vapours are raised to a still higher temperature, chloride of sulphur and protochloride of carbon are formed. Sometimes there are appearances as if a carburet of sulphur were formed, but of this I have not satisfied myself.

Phosphorus at low temperatures melts and unites with the substance, without any decomposition. If heated in the vapour of the substance, but not too highly, it takes away chlorine, and forms the protochlorides of phosphorus and carbon. If heated more highly, it frequently inflames in the vapour with a brilliant combustion, and abundance of charcoal is deposited. Sometimes I have had the charcoal left in films stretching across the tubes, and occupying the space where the flame passed. The appearance is then very beautiful.

When phosphorus is heated with the vapour of the substance over mercury, so as not to inflame in it, there is generally a small portion of muriatic acid gas formed. If great care be taken, this is in very minute quantity; and its variable proportion sufficiently shows, that the hydrogen which forms it does not come from the substance. I am induced to believe that it is derived from moisture adhering to the phosphorus. The action of iodine on phosphorus shows, that it is very difficult to dry the latter substance perfectly.

A stick of phosphorus put into the alcoholic or etherial solution of the perchloride did not exert any action upon it.

Charcoal heated in the vapour of the substance appears to have no action upon it.

Most of the metals decompose it at high temperatures. Potassium burns brilliantly in the vapour, depositing charcoal, and forming chloride of potassium. Iron, zinc, tin, copper, and mercury, act on it at a red heat, forming chlorides of those metals, and depositing charcoal; and when the experiments are made with pure substances, and very carefully, no other results are obtained. Some of the substance was passed over iron turnings heated in a glass tube. At the commencement of the sublimation of the chloride through the hot iron, the common air of the vessels was expelled, and received in different tubes; but before one-third of the substance had been passed, all liberation of gas ceased, and the remainder was decomposed by the iron, without the production of any gaseous matters. The different portions of air that were thrown out being examined, the first proved to be common air, and the last carbonic oxide. This had resulted, probably, from the action of the chlorine on the lead of the glass tube. An evident action had taken place, and the oxygen evolved, meeting with the liberated carbon, would produce the carbonic oxide. This experiment has been repeated several times with the same results.

When the perchloride of carbon is heated with metallic oxides, different results are produced according to the proportions of oxygen in the oxides. The peroxides, as of mercury, copper, lead, and tin, produce chlorides of those metals, and carbonic acid; and the protoxides, as those of zinc, lead, &c. produce also chlorides; but the gaseous products are mixtures of carbonic acid and carbonic oxide. I have frequently perceived the smell of phosgene gas on passing the chloride over oxide of zinc; and as the substance easily liberates chlorine at high temperatures, it will be readily seen how a small portion of that gas may be formed. It also happens, sometimes, that the protoxides become blackened from the deposition of charcoal.

When the vapour of the chloride is passed over lime, baryta, or strontia, heated red hot, a very vivid combustion is produced. The oxygen and the chlorine change places, and both the metals and the carbon are burnt. Chlorides are produced, carbonic acid is formed and absorbed by the undecomposed parts of the earths, and carbon is deposited. In these experiments no carbonic oxide is produced. When passed over magnesia, there is no action on the earth, but the perchloride of carbon is converted by the heat into protochloride.

In these experiments with the oxides no trace of water could be perceived.

Having thus far described the properties of the substance, I shall now give the reasons which induce me to consider it a true chloride of carbon, and shall endeavour to assign its composition. My first object was to ascertain whether hydrogen existed in it or not. When phosphorus is heated in it, a small quantity of muriatic acid is generally formed; but doubt arises as to the cause of its production, from the circumstance that the phosphorus, as already mentioned, may be the source of the hydrogen. When potassium is heated in the vapour of the substance, there is generally a small expansion of volume, and inflammable gas produced; but it is very difficult to cleanse potassium both from naptha and an adhering crust of moist potash; and either of these, though in extremely minute quantities, would give fallacious results.

A more unexceptionable experiment made with iron has been already described; and the inferences from it are against the presence of hydrogen in the compound.

Some of the substance in vapour was electrized over mercury by having many hundred sparks passed through it. Calomel was formed, and carbon deposited. A very minute bubble of gas was produced, but it was much too small to interfere with the conclusions drawn respecting the binary nature of the compound; and was probably caused by air that had adhered to the sides of the tube when the mercury was poured in.

The most perfect demonstration that the body contains no hydrogen, and indeed of its nature altogether, is obtained from the circumstances which attend its formation. When the fluid compound of chlorine and olefiant gas is acted on by chlorine and solar light in close vessels, although the whole of the chlorine disappears, yet there is no change of volume, its place being occupied by muriatic acid gas. Hence, as muriatic acid gas is known to consist of equal volumes of chlorine and hydrogen, combined without condensation, it is evident that half the chlorine introduced into the vessel has combined with the elements of the fluid, and liberated an equal volume of hydrogen; and as, when the chloride is perfectly formed, it condenses no muriatic acid gas, a method, apparently free from all fallacy, is thus afforded of ascertaining its nature.

I have made many experiments on given volumes of chlorine and olefiant gases. A clean dry retort was fitted with a cap and stop-cock. Its capacity was 25·25 cubic inches. Being exhausted by the air-pump, it was filled with nitrogen (24·25 cubic inches being required), and being again exhausted, 5 cubic inches of olefiant gas, and 10 cubic inches of chlorine, were introduced. It was then set aside for half an hour, that the fluid compound might form, and afterwards being placed again over a jar of chlorine, 19·25 cubic inches entered; so that the condensation had been as nearly as possible 10 cubic inches, or twice the volume of the olefiant gas (barometer, 29·1 inches). It was now placed for the day (Oct. 18) in the rays of the sun; but the weather was not very fine. In the evening the solid crystalline substance had formed in abundance, and very little fluid remained. When placed over chlorine, not the slightest change in volume had been produced. The stop-cock was now opened under mercury, and a small portion of the metal having entered, it was agitated in the retort, to absorb the chlorine; the neck of the retort was left open under the mercury all night, and the whole agitated from time to time. Next morning (barometer 29·6) the mercury which had entered, being passed into the neck of the retort, stood at a certain mark six inches above the level of the mercury in the trough, occupying 1·25 cubic inch, and leaving 24 cubic inches filled by the expanded muriatic acid gas and nitrogen. These volumes corrected to the pressure of 29·1 inches give 5·78 cubic inches for the chlorine absorbed, and 19·47 cubic inches for the muriatic acid gas, &c. These absorbed by water left 1·2 cubic inch of nitrogen; so that the gases in the retort, after the action of solar light, were:

	Cubic inches.
Muriatic acid gas	18·27
Chlorine	5·78
Nitrogen, &c.	1·2

and before that action,

Chlorine	29·25
Olefiant gas	5·0
Nitrogen	1·0

Hence 23·47 cubic inches of chlorine had disappeared, and 9·13 of these had entered into combination with an equal volume of 9·13 cubic inches of hydrogen liberated from the five cubic inches of olefiant gas, to form muriatic acid; and, consequently, 14·34 cubic inches of chlorine remained combined with the carbon of the five cubic inches of olefiant gas. Here, the volume of chlorine actually employed is not quite five times that of the olefiant gas, nor the volume of muriatic acid gas produced, equal to four times that of the olefiant gas; but they approximate; and when it is remembered that the conversion was not quite perfect, and that the gases used would inevitably contain a slight portion of impurity, the causes of the deficiency can easily be understood.

In other experiments made in the same way, but with smaller quantities, more accurate results were obtained: 1 cubic inch of olefiant gas with 12·25 cubic inches of chlorine, produced by the action of light 3·67 cubic inches of muriatic acid gas, 4·963 of the chlorine having been used. 1·4 cubic inch of olefiant gas with 12·5 cubic inches of chlorine produced 5·06 cubic inches of muriatic acid gas, 6·7 cubic inches of chlorine having been used. Other experiments gave very nearly the same results; and I have deduced from them, that one volume of olefiant gas requires five volumes of chlorine for its conversion into muriatic acid and chloride of carbon; that four volumes of muriatic acid gas are formed; that three volumes of chlorine combine with the two volumes of carbon in the olefiant gas to form the solid crystalline chloride; and that, when chlorine acts on the fluid compound of chlorine and olefiant gas, for every volume of chlorine that combines, an equal volume of hydrogen is separated.

I have endeavoured to verify these proportions by analytical experiments. The mode I adopted was, to send the substance in vapour over metals and metallic oxides at high temperatures. Considerable care is requisite in such experiments; for if the process be carried on quickly, a portion of fluid chloride of carbon is formed, and escapes decomposition. The following are two results from a number of experiments agreeing well with each other.

Five grains were passed over peroxide of copper in an iron tube, and the gas collected over mercury; it amounted to 3·9 cubic inches, barometer 29·85; thermometer 54° Fahr. Of these nearly 3·8 cubic inches were carbonic acid, and rather more than ·1 of a cubic inch was carbonic oxide. These are nearly equal to ·5004 of a grain of carbon. Hence, 100 of the chloride would give 10 of carbon nearly, but by calculation 100 should give 10·19. The difference is so small as to come within the limits of errors in experiment.

Five grains were passed over peroxide of copper in a tube made of green phial glass, and the chlorine estimated in the same manner as before. 17·7 grains of chloride of silver were obtained equal to 4·36 grs. of chlorine. This result approaches much nearer to the calculated result than the former; but there had still been action on the tube, and a minute portion of the substance had passed undecomposed, and condensed at the opposite end of the tube in crystals.

Experiments made by passing the perchloride over hot lime or barytes, promise to be more accurate and easy of performance. In the mean time, the above analytical results will, perhaps, be considered as strong corroboration of the opinion of the nature of the compound, deduced from the synthetical experiments; and the composition of the perchloride of carbon will be

3 proportions of chlorine........ =	100·5
2 ditto carbon .... =	11·4
	111·9

Protochloride of Carbon.

Having said so much on the nature of the perchloride of carbon, I shall have less occasion to dwell on the proofs that the compound I am about to describe, is also a binary combination of carbon and chlorine.

When the vapour of the perchloride of carbon is heated to dull redness, chlorine is liberated, and a new compound of that element and carbon is produced. This is readily shown by heating the bottom of a small glass tube, containing some of the perchloride in a spirit lamp. The substance at first sublimes, but as the vapour becomes heated below, it is gradually converted into protochloride, and chlorine is evolved.

It is not without considerable precaution that the protochloride of carbon can be obtained pure; for though passed through a great length of heated tube, part of the perchloride frequently escapes decomposition. The process I have adopted is the following: some of the perchloride is introduced into the closed end of a tube, and the space above it, for 10 or 12 inches, filled with small fragments of rock crystal; the part of the tube beyond this is then bent up and down two or three times, so that the angles may form receivers for the new compound; then heating the tube and crystal to bright redness, and dipping the angles in water, the perchloride is slowly sublimed by a spirit lamp, and, on passing into the hot part of the tube, is decomposed; a fluid passes over, which is condensed in the angles of the tube, and chlorine is evolved; part of the gas escapes, but the greater portion is retained in solution by the fluid, and renders it yellow. Having proceeded thus far, by the careful application of a lamp and blow-pipe, the bent part of the tube may be separated from that within the furnace, and the end closed, so as to form a small retort; and on distilling the fluid four or five times from one angle to the other, all the chlorine may be driven off without any loss of the substance, and it becomes limpid and colourless. It still, however, always contains some perchloride, which has escaped decomposition; and, to separate this, I have boiled the fluid until the tube was nearly full of its vapour, and then closing the end that still remained open, by a lamp and blowpipe, have afterwards left the whole to cool. It is then easy, by collecting all the fluid into one end of the tube, and introducing that end through a cork into a receiver, under which a very small flame is burning, to distil the whole of the fluid at a temperature very little above that of the atmosphere. The solid chloride being less volatile does not rise so soon, and the pure protochloride collects at the external end of the tube. To ascertain its purity, a drop may be placed on a glass plate; it will immediately evaporate, and if it contains perchloride, that substance will be left behind; otherwise, no trace will remain on the glass. The presence or absence of free chlorine may be ascertained by dissolving a little of the fluid in alcohol or ether, and testing by nitrate of silver.

The pure protochloride of carbon is a highly limpid fluid, and perfectly colourless. Its specific gravity is 1·5526. It is a non-conductor of electricity. I am indebted to Dr. Wollaston for the determination of the refractive power of this chloride, and for the approximation to the refractive power given of the perchloride. In the present case it is 1·4875, being very nearly that of camphor. It is not combustible except when held in a flame, as of a spirit lamp, and then it burns with a bright yellow light, much smoke, and fumes of muriatic acid.

It does not become solid at the zero of Fahrenheit's scale. When its temperature is raised under the surface of water to between 160° and 170°, it is converted into vapour, and remains in that state until the temperature is lowered. When heated more highly, as by being passed over red hot rock crystal in a glass tube, a small portion is always decomposed; nearly all the fluid may, however, be condensed again, but it passes slightly coloured, and the tube and crystal are blackened on the surface by charcoal. I am uncertain whether this decomposition ought not to be attributed rather to the action of the glass at this high temperature than to the heat alone.

It is not soluble in water, but remains at the bottom of it in drops, for many weeks, without any action.

It is soluble in alcohol and ether, and the solutions burn with a greenish flame, evolving fumes of muriatic acid.

It is soluble in the volatile and fixed oils. The volatile oils containing it burn with the emission of fumes of muriatic acid. When the solutions of it in the fixed oils are heated, they do not blacken or evolve fumes of muriatic acid. It is, therefore, probable, that when this happens with the solution of the perchloride in fixed oils, it is from its conversion by the heat into protochloride and the liberation of chlorine.

It is not soluble in alkaline solutions, nor do they act on it in some days. Neither is it at all soluble in, or affected by, strong nitric, muriatic, or sulphuric acids.

Solutions of silver do not act on it.

Oxygen decomposes it at high temperatures, forming carbonic oxide, or acid, and liberating chlorine.

Chlorine dissolves in it in considerable quantity, but has no further action, or only a very slow one in common day light; on exposure to solar light, a different result takes place. I have only had two days, and those in the middle of November, on which I could expose the protochloride of carbon in atmospheres of chlorine to solar light; and hence the conversion of the whole of the protochloride was not perfect; but at the end of those two days the retorts containing the substances were lined with crystals, which, on examination under the microscope, proved to be quadrangular plates, resembling those of the perchloride of carbon. There were also some rhomboidal crystals here and there. After the formation of these crystals, there was considerable absorption in the retort; hence chlorine had combined; and the gas which remained was chlorine unmixed with any thing else, except a slight impurity. The solid body, on examination, was found to be volatile, soluble in alcohol, precipitable by water, and had the smell and other properties of perchloride of carbon. Hence, though heat in separating chlorine from the perchloride of carbon produces its decomposition, light occasions its reproduction.

It dissolves iodine very readily, and forms a brilliant red solution, similar in colour to that made by putting iodine into sulphuret of carbon, or chloric ether. It does not exert any further action on iodine at common temperatures.

An electric spark passed through a mixture of the vapour of the chloride with hydrogen, does not cause any detonation, but when a number are passed, the decomposition is gradually effected, and muriatic acid is formed. When hydrogen and the vapour of the protochloride are passed through a red hot tube, there is a complete decomposition effected, muriatic acid gas being formed, and charcoal deposited. The mixed vapour and gas burn with flame as they arrive in the hot part of the tube. The vapour of the protochloride detonates readily by the electric spark with a mixture of oxygen and hydrogen gases, and a complete decomposition is effected. It will not detonate with the vapour of water.

Sulphur and phosphorus both dissolve in it, but exert no decomposing action at temperatures at, or below, the boiling point of the chloride. The hot solution of sulphur becomes a solid crystalline mass by cooling. Phosphorus decomposes it at a red heat.

Its action on metals is very similar to that of the perchloride. When passed over them at a red heat, it forms chlorides, and liberates charcoal. Potassium does not act on it immediately at common temperatures; but, when heated in its vapour, burns brilliantly, and deposits charcoal.

When passed over heated metallic oxides, chlorides of the metals are formed, and carbonic oxide, or carbonic acid, according to the state of oxidation of the metal. When its vapour is transmitted over heated lime, baryta, or strontia, the same brilliant combustion is produced as with the perchloride.

While engaged in analyzing this chloride of carbon, for the purpose of ascertaining the proportions of its elements, I endeavoured, at first, to find how much chlorine was liberated from a certain weight of perchloride during its conversion into protochloride, and for this purpose distilled the perchloride through red hot tubes into solution of nitrate of silver, receiving the gas into tubes filled with and immersed in the same solution; but I could never get accurate results in this way, from the difficulty of producing a complete decomposition, and also from the formation of chloric acid. Five grains of perchloride distilled in this manner gave 4·3 grains of chloride of silver, which are equivalent to 1·06 grain of chlorine; but some of the chloride evidently passed undecomposed, and crystallized in the tube.

2·7 grains of the pure protochloride were passed over red hot pure baryta in a glass tube: a very brilliant combustion with flame took place, chloride of barium and carbonic acid were produced, and a little charcoal deposited. When the tube was cold, the barytes was dissolved in nitric acid, and the chlorine precipitated by nitrate of silver. 9·4 grains of dry chloride of silver were obtained = 2·32 grains of chlorine.

Other experiments were made with lime, which gave results very near to this, the quantity of chloride being rather less.

Three grains of pure protochloride were passed over peroxide of copper heated red hot in an iron tube, and the gas received over mercury. 3·5 cubic inches of carbonic acid gas came over mixed with ·1 of a cubic inch of common air. These 3·5 cubic inches are nearly equal to ·449 of a grain of carbon.

These experiments indicate the composition of the fluid chloride of carbon to be one proportion of chlorine and one of carbon, or 33·5 of the former, and 5·7 of the latter. The difference between these theoretical numbers, and the results of the experiments, is not too great to have arisen from errors in working on such small quantities of the substance.

A mixture of equal volumes of oxygen and hydrogen was made, and two volumes of it detonated with the vapour of the protochloride in excess over mercury by the electric spark. The expansion was very nearly to four volumes; of these, two were muriatic acid, and the rest pure carbonic oxide: and calomel had been formed, its presence being ascertained by potash. Hence it appears, that one volume of hydrogen and half a volume of oxygen had decomposed one proportion of the protochloride, forming the two volumes of muriatic acid gas and one volume of carbonic oxide; and that at the intense temperature produced within the tube by the inflammation, the rest of the oxygen and the mercury had decomposed a further portion of the substance, giving rise to the second volume of the carbonic oxide, and to the calomel.

A mixture of two volumes of hydrogen and one volume of oxygen was made, and three volumes of it detonated with the vapour, as before. After cooling, the expansion was to six volumes, four of which were muriatic acid, and two carbonic oxide. There was no action on the mercury in this experiment. Again, five volumes of the same mixture being detonated with the vapour of the substance, expanded to 9·75 volumes, of which 6·25 were absorbed by water and were muriatic acid, and 3·5 were carbonic oxide mixed with a very small portion of air introduced along with the fluid chloride. These experiments, I think, establish the composition of the protochloride of carbon, and prove that it contains one proportion of each of its elements.

From a consideration of the proportions of these two chlorides of carbon, it seems extremely probable that another may exist, composed of two proportions of chlorine combined with one of carbon. I have searched assiduously for such a compound, but am undecided respecting its production. When the fluid protochloride was exposed with chlorine to solar light, crystals were formed, as before described. The greater number of these were certainly the perchloride first mentioned in this paper; but when the retort was examined by a microscope, some rhomboidal crystals were observed here and there among those of the usual dendritic and square forms. These may, perhaps, be the real perchloride; but I had not time, before the season of bright sunshine passed away, to examine minutely what happens in these circumstances; and must defer this, with many other points, till the next year brings more favourable weather.

Compound of Iodine, Carbon, and Hydrogen.

The analogy which exists between chlorine and iodine naturally suggested the possible existence of an iodide of carbon, and the means which had succeeded with the one element offered the best promise of success with the other.

Iodine and olefiant gas were put in various proportions into retorts, and exposed to the sun's rays. After a while, colourless crystals formed in the vessels, and a partial vacuum was produced. The gas in the vessels being then examined, was found to contain no hydriodic acid, but only pure olefiant gas. Hence, the effect had been simply to produce a compound of the iodine with the olefiant gas.

The new body formed was obtained pure by introducing a solution of potash into the retort, which dissolved all the free iodine; the substance was then collected together and dried. It is a solid white crystalline body, having a sweet taste and aromatic smell. It sinks readily in sulphuric acid of specific gravity 1·85. It is friable; is not a conductor of electricity. When heated, it first fuses, and then sublimes without any change. Its vapour condenses into crystals, which are either prismatic, or in plates. On becoming solid after fusion, it also crystallizes in needles. The crystals are transparent. When highly heated it is decomposed, and iodine evolved. It is not readily combustible; but when held in the flame of a spirit lamp, burns, diminishing the flame, and giving off abundance of iodine, and some fumes of hydriodic acid. It is insoluble in water, or in acid and alkaline solutions. It is soluble in alcohol and ether, and may be obtained in crystals from these solutions. The alcoholic solution is of a very sweet taste, but leaves a peculiarly sharp biting sensation on the tongue.

Sulphuric acid does not dissolve it. When heated in the acid to between 300° and 400°, the compound is decomposed, apparently by the heat alone; and iodine and a gas, probably olefiant gas, are liberated. Solution of potash acts on it very slowly, even at the boiling point, but does gradually decompose it.

This substance is evidently analogous to the compound of olefiant gas and chlorine, and remarkably resembles it in the sweetness of its taste, though it differs from it in form, &c. It will with that body form a new class of compounds, and they will require names to distinguish them. The term chloric ether, applied to the compound of olefiant gas and chlorine, did not at any time convey a very definite idea, and the analogous name of iodic ether would evidently be very improper for a solid crystalline body heavier than sulphuric acid. Mr. Brande has suggested the names of hydriodide of carbon, and hydrochloride of carbon, for these two bodies. Perhaps as their general properties range with those of the combustibles, while the specific nature of the compound is decided by the supporter of combustion which is in combination, the terms of hydrocarburet of chlorine, and hydrocarburet of iodine, may be considered as appropriate for them.

As yet I have not succeeded in procuring an iodide of carbon, but I intend to pursue these experiments in a brighter season of the year, and expect to obtain this compound.

Article IV.

Experiments to determine the Atomic Weight of various Metals and Acids. By Thomas Thomson, MD. FRS.

I have in three preceding papers, one of which appeared in vol. xvi. First Series; and the other two in vol. i. of the present Series of the *Annals of Philosophy* given a set of experiments, which appear to me to fix, with a degree of accuracy that cannot easily be surpassed, the atomic weights of 20 different chemical substances. I have also ascertained the weight of the combinations with oxygen which these different substances form. These compounds, including several bodies, whose atomic weight was determined in my paper on the *specific gravity of gases*, make the number of important chemical bodies, whose atomic weights may now be considered as fixed with accuracy, amount to 65. Indeed this number would be very materially increased if we were to include the sulphurets and phosphurets and salts which may be formed from these bodies—all of which may be deduced with perfect precision from the atomic weights laid down in these papers.*

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* In the Annales de Chimie et de Physique for July, 1820, tom. xiv. p. 321, M. Gay-Lussac has done me the honour to make some remarks upon the opinions which I advanced in the *Annals of Philosophy*, xv. 227, on the Composition of Phosphorous and Phosphoric Acids. It cannot be finally admitted, he says, that the oxygen in these two acids is as 1 to 2, till I lay before the chemical world the experiments proving that phosphuretted hydrogen gas contains exactly its own volume of hydrogen gas, as MM. Thenard and Gay-Lussac in their Recherches Physico-Chimiques, i. 214, have shown, that it contains about 1½ time its volume of hydrogen. I shall take this opportunity of satisfying the doubts of this very ingenious chemist.

In my paper on Phosphuretted Hydrogen Gas, printed in the *Annals of Philosophy*, viii. 87, and a translation of which was inserted in the Ann. de Chim. et de Phys. ii. 297, I have related a series of experiments which I made in order to determine this point. I showed that the phosphorus might be removed by the cautious introduction of oxygen gas in narrow tubes, by electric sparks, and by heating it along with sulphur. In all these trials, it was deprived of its phosphorus without any change of bulk whatever. Heating sulphur in this gas easily converts it into sulphuretted hydrogen gas without any change of volume. These experiments cannot leave any doubt respecting the truth of the fact, that phosphuretted hydrogen contains exactly its volume of hydrogen.

The experiment related by Thenard and Gay-Lussac in their Recherches Physico-Chimiques, i. 214, consisted in heating potassium in phosphuretted hydrogen gas. The phosphorus was separated, and 100 volumes of the gas became 149 volumes. We have not sufficient information given us by the authors to be able to explain it. When potassium is heated in pure phosphuretted hydrogen, the volume of the gas is not altered. When it is heated in bihydroguret of phosphorus, or the hydrophosphoric gas of Davy, the bulk of the gas is just doubled. It is probable that the gas used by these gentle-

I propose, in the present essay, to give an account of the experiments which appear to me to determine the exact atomic weight of 12 other bodies, some of which are metallic, and others acids. I shall begin, as I have hitherto done, by exhibiting in a table the atomic weights of these bodies as already laid down by Berzelius, the only other person who has published experiments to determine the weights of these bodies. Beside the numbers of Berzelius, I shall place those which I have obtained by my own experiments.

	Berzelius.		Thomson.		Berzelius reduced by dividing by 2.
Bismuth	17·738	....	9·0	....	8·869
Tin	14·7058	....	7·25	....	7·3529
Antimony	16·1290	....	5·5	....	8·0645
Mercury	25·3160	....	25·0		
Arsenious acid.	12·4077	....	6·75	....	6·20385
Boracic acid ..	26·965	....	2·75		
Oxalic acid. ..	27·106	....	4·5		
Tartaric acid ..	8·3449	....	8·375		
Citric acid	7·2785	....	7·25		
Benzoic acid ..	15·0955	....	15·0		
Succinic acid .	6·2785	....	6·25		
Acetic acid. ..	6·4112	....	6·25		

Several of Berzelius's numbers require to be divided by 2 in order to be compared with mine. The result of this division will be seen in the third column of the table; and it will there appear that his numbers for bismuth, tin, and mercury, differ but little from mine. The difference in the number for antimony and arsenious acid is greater. In both of these I suspect that Berzelius was misled by theoretical considerations; at least I

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men in their experiment was a mixture of equal volumes of these two gases. Supposing their gas to have been pure phosphuretted hydrogen at first, if it was left standing on water for a day or two, it would be partly converted into bihydroguret of phosphorus.

If M. Gay-Lussac will take the trouble to repeat the experiments which I have related in the paper above referred to, if he peruse what I have said on the subject in the *Annals of Philosophy*, xvi. 261, and i. (second series) p. 9, I flatter myself that any doubts which he may still retain will be removed.

I cannot avoid noticing here, though not exactly the proper place, a remark of M. Gay-Lussac upon my analysis of subbichloride of sulphur, an abstract of which he has done me the honour to translate and to publish in the Ann. de Chim. et de Phys. xiv. 322. He says that I have greatly underrated the quantity of sulphur, as sulphuric acid must have been formed during the action of water on the subbichloride in considerable quantity. Had he taken the trouble to peruse my analysis, he would have seen that I mixed the aqueous solution with nitrate of barytes, that no immediate precipitate fell, but that in 24 hours I obtained a quantity of white powder weighing two grains. This I consider a sufficient proof that no sulphuric acid was contained in the liquid. M. Gay-Lussac might have attacked the explanation which I gave of the phenomena; but the analysis is quite independent of that explanation, and was completed before I thought of theorising on the subject. I still consider my explanation as the true one; but whether it be so or not, the analysis shows, I conceive, that the liquid examined was a compound of two atoms sulphur and one atom chlorine.

found that I could obtain results agreeing more nearly with each other than those which he describes.

I. *Bismuth.*

The salts of bismuth are all decomposed when they are placed in contact with water. This prevents the possibility of determining the weight of the oxide of bismuth in the same way as I ascertained that of the metals whose atomic weights have been given in the former essays. It seems needless to relate the great variety of unsuccessful attempts which I made with the nitrate and sulphate of bismuth. I was not more successful with the chloride.

After a very great number of experiments, I gave up the salts and the chloride in despair. I then dissolved a quantity of the bismuth of commerce in nitric acid, decomposed the nitrate by means of water, edulcorated the oxide, and reduced it to the metallic state by heating it in a covered crucible with black flux. Upon bismuth thus obtained (which I consider as pure), I made the following experiment: Nine grains of it were put into a platinum crucible, and dissolved in pure nitric acid. The solution was evaporated to dryness by a gentle heat, which was gradually raised to 600°; and, finally, to redness. The oxide of bismuth which remained weighed exactly 10 grains. Two repetitions of the experiment gave exactly the same result. Hence I conclude that an atom of bismuth weighs exactly 9, and that the weight of oxide of bismuth is 10. Hence it is obviously a compound of one atom bismuth + one atom oxygen; and not two atoms, as Berzelius supposes. Indeed it is not difficult to show, that 10 is the equivalent number for oxide of bismuth. Weigh 10 grains of oxide of bismuth in a platinum crucible, pour on it a quantity of pure sulphuric acid (the experiment will not succeed with the acid of commerce), digest the whole upon a sand-bath for 24 hours, then raise the heat gradually and slowly till the excess of sulphuric acid is evaporated. The weight of the sulphate of bismuth thus formed is exactly 15 grs. Now 5 is the weight of an atom of sulphuric acid. We see that 5 of sulphuric acid are just neutralized by 10 of oxide of bismuth; consequently 10 is the atomic weight of oxide of bismuth.

To leave no ambiguity with respect to the weight of oxide of bismuth, I put into a retort 15 grains of sulphate of bismuth, and 13·25 grains of chloride of barium with a sufficient quantity of distilled water. The retort was put upon the sand-bath, and kept for several days at the boiling temperature. It was then placed in a cool part of the laboratory, and the fluid allowed to settle. The clear liquid was neither precipitated by muriate of barytes, nor by sulphate of soda. Hence it contained neither sulphuric acid nor barytes. Now 13·25 of chloride of barium yield one atom of barytes, just capable of saturating one atom of sulphuric acid, which we have seen already was contained in 15 of sulphate of bismuth. This experiment, in conjunction with the two others, seems to me to leave no doubt that bismuth weighs 9, and oxide of bismuth 10.

Berzelius's number is only 0·131 less than 9. It nearly coincides with the atomic weight of bismuth which I deduced from some experiments made by me on bismuth many years ago. But I employed the bismuth of commerce in these experiments, which is not quite pure. This I consider as the cause of the deficiency in these old experiments.

II. *Tin.*

The only salt of tin which exhibits a regular crystalline form, at least as far as my experiments extend, is the muriate. But this salt contains a considerable quantity of water, from which it cannot be deprived without converting it into an insoluble chloride. The muriate is soluble in water; but we do not readily succeed in throwing down its muriatic acid by means of nitrate of silver; for when we add that salt to muriate of tin, the solution becomes muddy, and the liquid, even though set aside for months, never becomes transparent. We may analyze this muriate of tin by first throwing down the tin by means of an alkali, neutralising the liquid by nitric acid, and then precipitating the muriatic acid by means of nitrate of silver. But this mode of analysis is too complicated to give results to be depended on, when an error of one-eighth of a grain would render the whole useless.

I next tried whether the fuming liquor of Libavius, which has been shown to contain no water, would not afford an easy method of analysis. When it is poured into distilled water, the tin is precipitated in the state of a white hydrate, and the chlorine is converted into muriatic acid. I separated the tin, dried, and weighed it; threw down the muriatic acid by means of nitrate of silver; the chloride was washed, dried, fused, and then weighed. Two successive analyses made in this way did not exactly agree with each other; but the mean of both did not differ materially from the previous analysis of this bichloride, for which we are indebted to Dr. John Davy.

Foiled in these attempts to discover the exact weight of an atom of tin, I attempted to reduce the peroxide by heating it in a glass tube, and passing hydrogen gas over it; but I was not able in this way to reduce the whole tin to the metallic state: part was reduced, while a portion obstinately remained in the state of protoxide.

My experiments, though unsuccessful, led to the notion that the weight of an atom of tin was in all probability 7·25. I, therefore, weighed 7·25 grains of pure tin in a platinum crucible, dissolved it in weak nitric acid, evaporated the solution to dryness very slowly, and then gradually exposed the crucible to a red heat. The peroxide of tin formed in this way weighed exactly 9·25. This experiment being twice repeated, the result was the same. It enables us to conclude that an atom of tin weighs 7·25; and that peroxide of tin is a compound of one atom of tin + two atoms of oxygen; so that we have the weight of tin and its oxides as follows:

Tin	7·25
Protoxide of tin	8·25
Peroxide of tin	9·25

III. *Antimony.*

We are indebted to Proust for the first accurate ideas respecting the oxides of antimony. Berzelius, in his elaborate experiments on antimony, published in vol. xxxiv. of Nicholson's Journal, has made a very near approximation to the true composition of these oxides. I have repeated both the experiments of Proust and Berzelius, and have been guided by them to the experiments which I am going to relate, and which I consider as leaving little doubt respecting the true atomic weight of antimony and its oxides. That I might operate upon antimony free from every sensible quantity of impurity, I dissolved the antimony of commerce in nitromuriatic acid, and precipitated the peroxide by means of water. This oxide was well washed, dried, mixed with black flux, and exposed to a red heat in a covered crucible. By this process, I obtained a button of antimony in which I could not detect any sensible quantity of foreign matter. This button was softer than common antimony, and its specific gravity was only 6·424 at the temperature of 60°.

Of this antimony I weighed 5·5 grains (which, as I concluded from preceding trials, represented the weight of an atom of antimony, at least very nearly), put it into a platinum crucible, and dissolved it by the assistance of heat in pure nitric acid. The solution was evaporated to dryness, and exposed for some hours to a heat of 500°. A yellow powder remained, exhibiting the well-known properties of peroxide of antimony, and weighing 7·5 grains. This experiment was repeated four times with exactly the same result. It is obvious from it, that peroxide of antimony is a compound of 5·5 antimony + 2 oxygen; or (as an atom of oxygen is represented by 1) of 1 atom antimony + 2 atoms oxygen. Therefore, an atom of antimony weighs exactly 5·5.

If we take 7·5 grains of peroxide of antimony and expose them to a red heat, the colour changes from *yellow* to *white*, and the weight diminishes to 7. I repeated this experiment four times with precisely the same result. We see from it (as had been already established by Berzelius) that when peroxide of antimony is changed into white oxide, it loses the fourth part of its oxygen. If we make the experiment in a small green glass retort (employing 100 grains of peroxide), and collect the gaseous products, we shall obtain very little short of 19 cubic inches of oxygen gas.

It cannot be doubted that protoxide of antimony, which we obtain by dissolving this metal in muriatic acid, and throwing it down by water, is a compound of one atom antimony + one atom oxygen. Of consequence, its weight must be 6·5. This oxide has a grey colour, and is rather dark. The white oxide must be a combination of one atom of protoxide and one atom of peroxide; for

1 atom protoxide .... = 6·5
1 atom peroxide. .... = 7·5
2)14·0
7·0

= 1 atom of white oxide.

The composition of the different oxides of antimony may be thus stated:

	Antimony.		Oxygen.
Protoxide	5·5	+	1·0
White oxide	5·5	+	1·5
Yellow oxide.	5·5	+	2·0

The protoxide of antimony dissolves in acids, and, therefore, possesses the characters of a base. I have never been able, however, to neutralize any acid by means of it. From the experiments of Berzelius, it appears that the peroxide combines with bases, and, therefore, is entitled to the name of acid. He has given it the name of antimonic acid. He gives the same account of the white oxide of antimony. I do not mean to deny the accuracy of his experiments, though I have not been so successful in my attempts to repeat them as I could wish. I did not succeed in obtaining a definite compound of the white oxide of antimony, though I have some reasons for suspecting that it is this oxide that enters into the composition of tartar emetic.

The red substance formerly distinguished by the name of *kermes mineral* is, I find, a hydrosulphuret of antimony composed of one atom sulphuretted hydrogen and one atom protoxide of antimony.

1 atom of sulphuretted hydrogen. . = 2·125
1 atom protoxide of antimony .... = 6·5
8·625

It is, therefore, composed of 2·125 of sulphuretted hydrogen + 6·5 protoxide of antimony; so that 100 parts of it consist of

Sulphuretted hydrogen	24·64
Protoxide of antimony	75·36
	100·00

It is easy to show that this is the true composition of kermes mineral. Put a quantity of it into a glass tube, and expose it to a heat of between 500° and 600°; a quantity of water is extricated, and common sulphuret of antimony remains. Thus by heat a double decomposition is produced, the oxygen of the oxide and the hydrogen of the gas unite together, and form water, while the metal and the sulphur combine and form the sulphuret. This experiment shows us that sulphuret of antimony is a compound of one atom antimony + one atom sulphur, or of

Antimony	5·5 or	$73\frac{1}{3}$
Sulphur	2·0	$26\frac{2}{3}$
	7·5	100

The analysis of sulphuret of antimony, which I published in the *Annals of Philosophy* (First Series), vol. iv. p. 99, was not quite accurate. I obtained

Antimony	73·77
Sulphur	26·23
	100·00

But it is very difficult to come nearer the truth when we follow the method by which that analysis was conducted. In rigid experiments all filtrations and transvasations must be avoided.

IV. *Mercury.*

The experiments of Sefstrom leave little doubt that the weight of an atom of mercury is 25; and that the protoxide is a compound of 100 mercury and 4 oxygen, and the peroxide of 100 mercury and 8 oxygen; for he actually obtained

	Mercury.		Oxygen.
Protoxide composed of	100	+	3·99
Peroxide	100	+	7·99

In these experiments the deviation was only one-fourth and one-eighth of per cent.

I tried a great many experiments to ascertain the point with precision; but the greater number of them failed. The crystals of nitrate of mercury contain water, from which I attempted to free them by placing them in the exhausted receiver of an air-pump with sulphuric acid; but this method was not successful.

I was obliged, therefore, to abandon the salts of mercury altogether. But the following experiment, which I repeated thrice, with exactly the same result, appears to me sufficiently decisive to settle the question respecting the composition of the oxides of mercury, if there be any chemists who entertain doubts on the subject.

If an atom of mercury weigh 25, and if the red oxide of mercury be a compound of one atom metal + two atoms oxygen, it is obvious that an integrant particle of it must weigh 27. To verify this, I put into an eight ounce phial 27 grains of peroxide of mercury, and poured upon it a quantity of muriatic acid sufficient to dissolve the oxide. The phial was then put upon a sand-bath (having its mouth stopped by a charcoal stopper), and left till the whole contents were evaporated to dryness. I then covered it up about two-thirds with sand, the temperature of which was gradually raised sufficiently high to sublime the corrosive sublimate towards the middle of the phial. The whole was then allowed to cool, and the phial being weighed, it was found that the corrosive sublimate weighed exactly 34 grains.

Now whether we take the view of the composition of corrosive sublimate deduced from Davy's theory of chlorine, or still adhere to the old notion that it is a compound of peroxide of mercury and muriatic acid, this experiment is decisive of the weight of an atom of mercury.

If we adhere to the old opinion, then corrosive sublimate is composed of

Peroxide of mercury	27
Muriatic acid	7
	34

But 3·5 (according to the old opinion) represents the weight of an atom of muriatic acid. The experiment shows us, therefore, that corrosive sublimate is a compound of two atoms muriatic acid and one atom peroxide of mercury; consequently peroxide of mercury must weigh 27.

If we adopt the theory of chlorine advanced by Davy, which I consider as much more consonant to the phenomena than the old opinion, then corrosive sublimate is a compound of

Mercury	25
Chlorine	9
	34

But an atom of chlorine weighs 4·5; therefore, 9 represents two atoms of chlorine; consequently corrosive sublimate is a compound of two atoms chlorine = 9, and one atom mercury = 25.

I have repeated this experiment several times, and it leaves no doubt in my mind that an atom of mercury weighs 25, and an atom of peroxide 27. Protoxide of course must weigh 26.

I made repeated attempts to convert mercury into a peroxide without any loss, but was not able to succeed. It may, perhaps, be worth while to relate a single experiment of this kind to enable the reader to judge of the degree of accuracy which I was able to reach by this method; because it may serve in some measure to account for the deviations from each other which we observe in the different experiments which have been published by different chemists to determine the composition of the oxides of mercury.

I put into a flask previously accurately weighed, and the weight marked on it by a diamond, 25 grains of mercury. A quantity of pure nitric acid capable of dissolving it was poured over it. After the solution was completed, the flask was placed on a sand-bath, and left in a moderate heat till the whole contents of it were evaporated to dryness. The flask was then exposed to a temperature of about 600°, and kept in it till all nitrous acid fumes ceased to exhale. The red oxide prepared in this way not being quite free from white particles, it was obvious that the nitrate had not been completely decomposed. I, therefore, heated a hessian crucible to redness, and introduced the flask into it, holding it at a little distance from the sides and bottom of the crucible. This occasioned the discharge of some more nitrous acid fumes, and the peroxide at the bottom of the flask had assumed a very fine red colour. I considered it in consequence as pure; but there had sublimed upon the sides of the flask traces of a yellow matter, which was tasteless, and dissolved in nitric acid without effervescence. It certainly did not amount to 1-100th part of a grain. I was, therefore, unable to determine its nature, though it was probably nothing but peroxide of mercury. I had covered the mouth of the flask with a piece of glass before I introduced it into the crucible. On this piece of glass a small portion of metallic mercury had sublimed. I found this portion to weigh 0·2 gr. The weight of the peroxide of mercury was 26·7 grs. Had the 0·2 gr. of sublimed mercury been peroxidized, it would have weighed 0·216 gr. Hence the weight obtained was:

Peroxide in flask	26·7
Ditto sublimed	0·216
	26·916
Loss	0·084
	27·000

Here the loss amounted to 0·084 gr. or about $\frac{1}{321}$ part of the whole. I have no doubt that this loss was owing to a small portion of mercury which had been sublimed, and had escaped, notwithstanding the glass cover on the flask.

I attempted to decompose calomel by means of a solution of caustic potash in the way recommended by Mr. Donovan; but did not obtain satisfactory results; because I was obliged, in order to dry the protoxide completely, to expose it to a temperature so high that it was partially reduced. If calomel be a compound of 1 atom mercury + 1 atom chlorine (about which there can be no doubt), it is evident that 29·5 grains of it, when decomposed by caustic potash, ought to give 26 grains of protoxide. The quantity which I obtained in repeated trials varied somewhat, but was in no case quite so high as 26 grains.

V. *Arsenious Acid.*

In an essay which is inserted in the *Annals of Philosophy* (Second Series), vol. i. p. 13, I have shown that arsenic acid is a compound of

Arsenic	4·75
Oxygen	3·00
	7·75

and that it is a compound of one atom arsenic + three atoms oxygen. I expressed a suspicion that arsenious acid was a compound of one atom arsenic + two atoms oxygen; and promised to endeavour to investigate the subject, and to lay the results before the public as soon as I obtained any which appeared to me decisive. It gives me pleasure to be now able to fulfil that promise.

When arsenious acid and quicklime are mixed together in a glass tube and heat applied, the acid is partly reduced to the metallic state, and partly converted into arsenic acid. It occurred to me that by determining the quantity of arsenic acid formed, and the quantity of arsenic sublimed, I should be able to deduce the composition of arsenious acid with precision. But the result of a good many trials made in this way was not satisfactory. The arsenious acid was seldom decomposed completely. A portion of it sublimed along with the arsenic, and prevented me from determining with certainty the quantity of metallic arsenic evolved. I was in consequence obliged to abandon this mode of experimenting.

I next tried whether it was possible to form a neutral arsenite. A quantity of arsenious acid was boiled in a solution of caustic potash; but I found it impossible in this way to deprive the liquid of the property of rendering cudbear paper purple. I next evaporated the whole to dryness, and by cautiously washing the residual matter with distilled water, I succeeded in removing all excess of alkali; but as soon as this salt was dissolved in water, crystals of arsenious acid deposited, and the liquid became alkaline. I was obliged, therefore, to abandon the arsenites, as I was unable to combine the acid and the base in definite proportions.

It was shown formerly that the true weight of an atom of arsenic acid is 7·75. Now if the oxygen in arsenious acid be to that in arsenic acid as 2 to 3, then it follows that an atom of arsenious acid weighs 6·75. To verify this supposition, I put 6·75 grs. of pure arsenious acid into a small retort, previously weighed, and the weight marked upon it by a diamond. Upon this I poured a mixture of 12 parts nitric acid, of the specific gravity 1·25, and 1 part muriatic acid, of the specific gravity 1·2, in such quantity as I knew from previous trials would be more than sufficient to convert the arsenious into arsenic acid. The retort being exposed to a moderate heat, the arsenious acid dissolved. The heat was continued very moderate till the whole liquid portion in the retort was expelled, and nothing remained but a white crust consisting of arsenic acid. The retort was now surrounded with hot sand, and exposed to a heat of between 500° and 600°, which was kept up for several hours. It was then allowed to cool and weighed. The weight of the arsenic acid was found exactly 7·75 grs. This experiment may be repeated as often as you please (provided the requisite precautions be taken) without the least variation.

It follows from it that the weights of arsenious and arsenic acids are respectively as follows:

Arsenious acid	6·75
Arsenic acid	7·75

If we take 4·75 grains of arsenic and convert them into arsenic acid by means of pure nitric acid, we shall find the weight of the arsenic acid formed, exactly 7·75 grains. It is obvious, therefore, that an atom of arsenic weighs 4·75, and that the constituents of the two acids of that base are:

	Arsenic.	Oxygen.
Arsenious acid	1 atom	+ 2 atoms
Arsenic acid	1	+ 3

So that arsenic exactly agrees with sulphur in the composition of its acids.

It is very likely that the black powder into which arsenic sometimes falls, when exposed to the air, is a protoxide of arsenic, or a compound of one atom arsenic + one atom oxygen. But I have been unable to verify this conjecture. The arsenic which I used did not change spontaneously into a black powder; and when I used an acid, whether the muriatic or the nitric, there was always some arsenious acid formed, while a portion of the arsenic retained its metallic state. Accident will probably some time or other make us acquainted with this oxide, if it exist.

VI. *Boracic Acid.*

My experiments to determine the weight of this acid have been attended with more difficulties than those on any other substance to which I have hitherto turned my attention; and after all my attempts to obtain accurate results, I have been obliged to rest satisfied with approximations, which, however, contract the limits of uncertainty within a narrow compass.

1. From Berzelius's experiments on borate of ammonia, it follows that it is composed of

Boracic acid	37·95
Ammonia	30·32
Water	31·73
	100·00[*]

Now this is equivalent to

Acid	2·659
Ammonia	2·125
Water	2·223

Now 2·125 is the weight of an atom of ammonia, and 2·223 approaches very nearly to 2·25, the weight of two atoms of water. There can be no doubt that borate of ammonia is a compound of one atom acid, one atom base, and two atoms water. Hence it follows that 2·659 approaches very nearly to the weight of an atom of boracic acid.

2. I put into a platinum crucible 100 grs. of dry boracic acid, obtained by decomposing borax by means of sulphuric acid, washing the precipitate well with distilled water, and drying it on a filter. The crucible was covered with a lid, and exposed for an hour to a strong red heat. The acid was fused into a glass which weighed 53·6 grains. Besides this, the cover of the crucible was coated with a varnish of boracic acid which weighed 1·9 gr. From this experiment we learn that the hydrate of boracic acid is composed of

Acid	55·5	or 2·806
Water	44·5	2·25
	100·0	

It would appear, therefore, that the hydrate of boracic acid is a compound of one atom acid + two atoms water. We see that the weight of an atom of acid deduced from this experiment (which I have repeated several times) is 2·806.

Now the mean weight of the atom of acid deduced from the analysis of borate of ammonia by Berzelius and my own analysis of hydrate of boracic acid is 2·7325. This quantity differs so little from 2·75 that I was induced to conclude that 2·75 represents the true weight of an atom of boracic acid. The object of the remaining experiments which I have to relate was to verify this opinion.

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[*] *Annals of Philosophy* (First Series), iii. 57.

3. 100 grains of crystallized borax were exposed in a platinum crucible to a heat sufficient to melt the salt. This heat was continued till the borax was reduced to a white mass; the crucible was then exposed to a strong red heat till the borax was melted into a transparent colourless glass. The loss of weight which the salt sustained was 47·2 grs. It appears from this experiment that crystallized borax contains 47·2 grs. of water of crystallization.

Twenty grains of anhydrous glass of borax were dissolved in distilled water by the assistance of heat, the solution was mixed with an excess of muriatic acid; and by repeated evaporations, the whole, or almost the whole, of the boracic acid was extracted from it. The weight of this acid (fused into a glass) was 5·1 grs. Now 100 grains of crystallized borax contain, as we have seen, 52·8 grains of the anhydrous salt. And 20 : 52·8 :: 5·1 : 13·464. It follows from this that 100 grs. of crystallized borax contain 13·464 grains of acid. Borax, therefore, must be composed as follows:

Acid	13·464
Soda	39·336
Water	47·200
	100·000

Now this is equivalent to

Acid	2·75
Soda	8·034
Water	9·640

Two atoms of soda are 8, which differs very little from 8·034. Eight atoms of water weigh 9. This is a little less than 9·64. The reason probably is, that when borax is exposed to a red heat, it is not the water alone which sublimes, but a portion of the salt passes off along with it. Indeed I have verified this suspicion by a direct experiment, and ascertained that the water driven off from borax by heat carries along with it a sensible quantity of the salt. Had I obtained from the 20 grs. of anhydrous borax 5·11 grs. of boracic acid instead of 5·1 grs. the quantity actually obtained, in that case the constituents of the salt would have borne to each other the exact ratio of 2·75 acid and 8 soda. Now when a substance is separated by a filter, and afterwards exposed to a red heat, I do not consider myself as capable of coming nearer the truth by the utmost attention which I can pay than 1-500th part of the whole. The deficiency of 0·01 gr. being greatly within that limit, I consider myself as entitled to infer, from the above analysis, that borax is a compound of 2·75 boracic acid and 8 soda, or of 1 atom boracic acid and 2 atoms soda. As for the watery constituent, it is not so well made out, though we can scarcely hesitate to admit that it amounts to just eight atoms. Borax then is composed of

1 atom boracic acid .... =	2·75
2 atoms soda.	8·0
8 atoms water....................	9 0
	19·75

So that an integrant particle of it weighs 19·75.

4. To verify this composition, I dissolved 19·75 grs. of crystallized borax in distilled water, and neutralized the soda by means of acetic acid. To get rid of any excess of acetic acid, I evaporated the liquid to dryness, and redissolved the residuum in water: 13·25 grains of chloride of barium were likewise dissolved in water, and the two solutions were mixed. It is evident that the 19·75 grains of borax contained (if my analysis was correct) exactly 2·75, or one atom of boracic acid; while 13·25 grains of chloride of barium, when dissolved in water, contain 9·75 grains, or one atom of barytes. Now as borate of barytes is an insoluble salt, I expected a double decomposition to have taken place, and that the whole borate of barytes would have precipitated, leaving the clear liquid incapable of being rendered muddy by muriate of barytes, sulphate of soda, or sulphuric acid. The result, however, was different. No precipitate whatever appeared, indicating that no decomposition had taken place. I, therefore, evaporated the whole to dryness, poured water on the dry residue, and after digesting for some time, poured it on a filter; there remained on the filter five grains of borate of barytes. The clear liquid which passed through the filter being evaporated to dryness, digested in water, and again filtered, left 7·1 grains of the same borate of barytes. The residual liquid was a third time evaporated to dryness, and treated as before; 0·3 gr. of borate of barytes were obtained. I repeated this process a fourth time, but could procure no more borate of barytes. Thus the whole borate of barytes obtained was:

	Grains.
By first filtration	5·0
By second filtration...................	7·1
By third filtration.	0·3
	12·4

Now if the 19·75 grs. of borax contained 2·75 grs. of boracic acid, and the 13·25 grs. of chloride of barium yield 9·75 grs. of barytes, it is obvious that the whole borate of barytes ought to have been 12·5 grs.; so that in the preceding experiment there seems to have been a loss sustained amounting to 0·1 gr. or less than one per cent. Now in an experiment consisting of three solutions, three evaporations to dryness, and three filtrations, I hold it beyond our present skill to guarantee a nearer approach to the truth than one per cent. This experiment then amounts to as complete a verification of the composition of borax as I am capable of exhibiting.

5. Borax appearing, from the preceding analysis, to be a compound of one atom of boracic acid and two atoms of soda, I was desirous to see what effect the addition of another atom of boracic acid would have upon the properties of this salt. I dissolved 19·75 grs. of it in hot water, and added 2·75 grains of glassy boracic acid. After the solution was completed, the liquid produced exactly the same effect upon cudbear paper that borax itself does. Another addition of 2·75 grs. of anhydrous boracic acid did not alter this property; nor was the property impaired by the addition of a third 2·75 grains; yet this solution must have contained 4 atoms of boracic acid = 11 grs. and only 2 atoms = 8 grs. of soda. We see from this that boracic acid does not possess the property of neutralising soda. Upon setting aside this solution in an open vessel, it gradually evaporated to dryness, but no appearance of any thing like regular crystals could be observed.

These experiments on boracic acid are the most accurate which I have been able to make. I have selected them out of a great number which do not offer better results than those which I have given, and which, therefore, I thought it unnecessary to transcribe. Though they do not absolutely decide the point, yet they render it exceedingly probable that an atom of boracic acid weighs 2·75, or exactly the same with carbonic acid. Borax then and carbon have the same atomic weight each, or 0·75; and boracic acid, like carbonic, is a compound of one atom boron + two atoms oxygen.

VII. *Oxalic Acid.*

Berzelius considers oxalic acid as a compound of 1 atom hydrogen + 12 atoms carbon + 18 atoms oxygen. According to this notion, an integrant particle of it should weigh 27·125. The number which he assigns, viz. 27·106, owes its slight difference from 27·125 to the weights which Berzelius has given to the atoms of carbon and hydrogen not being quite the same as mine. Carbon, according to him, weighs 0·7533; while hydrogen weighs only 0·062177. But when we come to examine the saline combinations of oxalic acid, we find that a much smaller weight of it than 27·125 is able to saturate an atom of each of the bases. Oxalate of potash, according to the statement of Berzelius himself, is composed of

Acid	43·37 or	4·595
Potash	56·63	6·0
	100·00	

Thus the equivalent number for oxalic acid in that salt is only 4·595 by Berzelius's own analysis. The same remark applies to all the oxalates which he has given in his table.

Now that we are acquainted with the true atomic weight of lime, and with the true composition of calcareous spar, there is no great difficulty in determining the true atomic weight of oxalic acid with rigid accuracy. The following experiments, which I have often verified, leave no doubts whatever upon the subject.

It is well known that oxalic acid usually is crystallized in flat four-sided rectangular prisms. These crystals constitute a definite compound of pure oxalic acid and water, and, therefore, are entitled to the name of hydrate of oxalic acid. As these crystals are always rigidly the same in their composition, it will be safest and most satisfactory to employ them in order to settle the equivalent number for oxalic acid. Nine grains of the crystals of oxalic acid were dissolved in a little water, and the solution was saturated with ammonia, gently evaporated to dryness, and the residual oxalate of ammonia redissolved in distilled water.

The reader will please to recollect, that I showed in a former paper that calcareous spar is composed of

Lime	3·5
Carbonic acid	2·75
	6·25

And that an atom of lime weighs 3·5.

6·25 grains of pure calcareous spar were dissolved in muriatic acid, the solution was gently evaporated to dryness, and the dry salt redissolved in distilled water. These two solutions were mixed together. Oxalate of lime immediately precipitated. The supernatant liquor, as soon as it became clear, was tested by oxalate of ammonia and muriate of lime; but it was not the least disturbed by either of these reagents. Hence it is evident that it contained neither oxalic acid nor lime; consequently the whole of these two substances which had been in solution had precipitated in the state of oxalate of lime.

It is clear from this experiment, that nine grains of crystallized oxalic acid contain a quantity of acid capable of exactly neutralizing 3·5 grs. of lime.

The oxalate of lime formed in this experiment was dried in the temperature of about 100°; in this state, it weighed 10·3 grs. It was now placed on the sand-bath, and exposed for two hours to a temperature at first of 450°, but rising gradually, to 500°, or, perhaps, a few degrees higher. The weight was now reduced to 8·03 grs. When thus reduced in weight, it still retained all the properties of oxalate of lime. Another hour's exposure to a heat of about 550° reduced the weight to 8·015 grs. Beyond this I did not urge the experiment, though I have little doubt that a temperature of 600° would have reduced the weight to exactly 8 grs. I was afraid of charring the salt, and thus depriving myself of the power of ascertaining whether the acid continued possessed of all its original characters. This experiment, which was several times repeated with exactly the same result, seems to me to leave no doubt that the oxalate of lime obtained from 9 grs. of crystallized oxalic acid and 6·25 grs. of calcareous spar, weighs, when perfectly freed from moisture, exactly 8 grs. Now 3·5 grs. of it are lime; consequently, the other 4·5 grs. are oxalic acid.

We see from this experiment, that 9 grains of the crystallized oxalic acid contain precisely 4·5 grs. of true acid; and that the equivalent number for oxalic acid is 4·5. Hydrate of oxalic acid then, or the crystals, are composed of

4·5 acid. .... = 1 atom,
4·5 water .... = 4 atoms.

Or they consist of 1 atom acid united to 4 atoms water.

We see further, that when oxalate of lime is dried at a temperature not exceeding 100°, it retains two atoms water; for it weighed 10·3 grs. Now

1 atom oxalate of lime .... = 8·00
2 atoms water .... = 2·25

10·25

So that a combination of two atoms water and 1 atom anhydrous oxalate of lime weighs 10·25, which differs only by 0·05 gr. or rather less than half a per cent. from 10·3, the number actually obtained.

The knowledge of this fact suggests a mode of determining the quantity of lime in mineral bodies, which I have been in the habit of following for some time past. The method is this. Throw down the lime by means of oxalate of ammonia, separate the oxalate of lime upon a filter, wash it, and dry it in a temperature not higher than 100. Ascertain its weight, and multiply it by $\frac{3{\cdot}5}{10{\cdot}25}$, or by 0·341; the product is the quantity of lime contained in the oxalate.

Since the equivalent number for oxalic acid is 4·5, there seems no reason to hesitate about considering it as a compound of two atoms carbon + three atoms oxygen; for

2 atoms carbon .... = 1·5
3 atoms oxygen.............. 3·0

4·5 = oxalic acid.

Berzelius obtained the small quantity of hydrogen which he found in it, because his salts had not been completely freed from water. Even the oxalate of lime which I dried at the temperature of 550° would have afforded traces of hydrogen, though the quantity would have been much less than Berzelius found; for its constituents (in consequence of the 0·015 of water still retained) would have been:

Hydrogen.	0·0016
Carbon.	1·5
Oxygen	3·0133
	4·515

Thus it would be a compound of about

1 atom hydrogen,
156 atoms carbon,
235 atoms oxygen.

These numbers are too high to be admitted, and show clearly the source of Berzelius's hydrogen.

Zinc is completely precipitated from the sulphate by oxalic acid. I find that the complete precipitation of this metal is not prevented by previously mixing the solution of sulphate of zinc with some sulphuric acid. The composition of the crystals of sulphate of zinc is as follows:

1 atom sulphuric acid	= 5·00
1 atom oxide of zinc	= 5·25
6 atoms water	= 6·75
	17·00

17 grs. of these crystals being dissolved in water, nine grains of crystallized oxalic acid were thrown into the solution, and the whole was well agitated for some time. A white powder immediately appeared, which gradually fell to the bottom, leaving a transparent colourless liquid swimming over it. This liquid reddened vegetable blues; but was neither precipitated by muriate of lime, nor carbonate of ammonia. Hence it contained neither oxalic acid, nor oxide of zinc. The oxalate of zinc, when washed and dried in the open air, weighed, in one experiment, 10·5 grs.; in another, 10·43 grs. This powder being exposed on a sand-bath to a temperature of 550° for several hours was reduced to the weight of 7·75 grs. In this state, it was a tasteless white powder, which dissolved without effervescence in muriatic acid, and when the solution was mixed with muriate of lime, a white precipitate fell. It is obvious from this, that it still contained oxalic acid. As it contained the whole oxide of zinc in 17 grs. of the sulphate, it must have consisted of

Oxide of zinc	5·25
Oxalic acid	2·5
	7·75

Consequently it was a compound of two atoms oxide of zinc and one atom oxalic acid. It appears from this that when oxalate of zinc is exposed to the temperature of 550°, it loses not merely its water, but likewise one half of its acid.

When we drop oxalic acid into a solution of common blue vitriol only one-half of the oxide of copper is precipitated in combination with oxalic acid; the remaining half is kept in solution, probably in consequence of the excess of acid present. Ammonia dissolves both oxalate of copper and oxalate of zinc.

VIII. *Tartaric Acid.*

The crystals of this acid, like those of oxalic, constitute a hydrate composed of one atom real acid + one atom water. The equivalent number for them is 9·5; consequently they are composed of

Tartaric acid	= 8·375
Water	= 1·125
	9·5

To verify the accuracy of this statement, it seems only necessary to give the following experiments:

1. 9·5 grs. of crystals of tartaric acid were dissolved in water, and saturated with ammonia. 6·25 grs. of calcareous spar were dissolved in muriatic acid, the solution was evaporated to dryness, and the muriate of lime redissolved in water. These two liquids being mixed together were very slowly evaporated to dryness. Crystals of tartrate of lime separated in abundance. Distilled water was poured upon this salt, and left upon it for 24 hours. It was then tested with oxalate of ammonia, but found to contain no lime. A portion of it being mixed with muriate of lime, and evaporated to dryness, the saline residue dissolved completely in distilled water, and, therefore, contained no tartrate of lime. Thus we see that 9·5 grs. of the crystals of tartaric acid contain exactly the quantity of acid necessary to saturate 3·5 grs. of lime.

When the tartrate of lime is dried at the temperature of 100°, it retains very little water; for its weight was only 12·3 grains. It undergoes decomposition at a much lower temperature than oxalate of lime; for it was partially destroyed in a heat, which I do not think exceeded 500°. Probably if I had dried it at the temperature of 212°, it would have been perfectly freed from water.

2. 9·5 grains of the crystals of tartaric acid were dissolved in

water, and saturated with ammonia: 20·75 grs. of dry nitrate of lead were dissolved in another portion of water, and the two solutions mixed. Tartrate of lead precipitated abundantly, and the supernatant liquor being tested with sulphate of soda and nitrate of lead exhibited no traces of containing either oxide of lead or tartaric acid. The tartrate of lead thus formed being washed and dried in a temperature not exceeding 100° weighed 22·4 grs. It was obviously a compound of one atom acid + one atom oxide of lead. Now

1 atom acid weighs	8·375
1 atom oxide of lead	14·0
	22·375

So that the weight exceeded the truth only by 0·025 gr. or not more than 1000th part. It is clear from this, that tartrate of lead, if we dry it at the temperature of 212°, will contain no water. This gives us the following formula for determining the quantity of tartaric acid in any soluble tartrate. Dissolve the salt in water, and precipitate the acid by nitrate of lead. Wash the precipitate, and dry it in the temperature of 212°. Weigh it, and multiply the weight by $\frac{8\cdot375}{22\cdot375}$, or by 0·374, the product is the quantity of tartaric acid contained in the tartrate.

IX. *Citric Acid.*

From the experiments made on this acid by Berzelius, there is reason to conclude, that the atomic weight of it is 7·25, and that the crystals are composed of 1 atom acid + 2 atoms water, or of

Acid	7·25
Water	2·25
	9·5

If this supposition be true, the equivalent number for the crystals of tartaric acid and citric acid is the same, or 9·5.

My attempts to verify this opinion by decomposing citrate of ammonia by means of muriate of lime did not lead to satisfactory results; I shall not, therefore, give an account of them. The following experiment I consider as decisive:

9·5 grs. of the crystals of citric acid were dissolved in water neutralised with ammonia, and then mixed with a solution of 20·75 grs. of nitrate of lead. It is worthy of attention that no precipitate appears, neither when citrate of ammonia is mixed with muriate of lime, nor with nitrate of lead. But when the mixture is slowly evaporated to dryness, the citrate of lead gradually separates, and is not again dissolved, though the dry residue be digested in distilled water. A portion of this clear liquid was drawn off and tested. When mixed with a solution of sulphate of soda, no precipitate appeared, so as to indicate the presence of lead. Into another portion of it some nitrate of lead was dropped, and the whole was evaporated to dryness. Distilled water being digested on the dry residue, a complete solution was effected. This indicates the absence of citric acid. Thus we see that the citric acid in 9·5 grs. of the crystals is just capable of saturating all the protoxide of lead which exists in 20·75 grains of nitrate of lead. This we know amounts to exactly 14 grs.

The citrate of lead formed in the preceding experiment was dried in a temperature not exceeding 100°. Its weight was exactly 21·25 grs. Now it contained 14 grs. of protoxide of lead; consequently the other 7·25 grs. must have been citric acid. Thus we see that an atom of citric acid weighs exactly 7·25, and an atom of the crystals 9·5. Citrate of lead, when dried at 100°, contains no water of crystallization; consequently, when we wish to analyze a citrate, we must dissolve a given weight of it in water, and throw down the acid by nitrate of lead and evaporation. When the precipitate is washed and dried at the temperature of 100°, its weight multiplied by $\frac{7\cdot25}{21\cdot25}$ or by 0·341, gives us the quantity of citric acid in the salt.

10. *Benzoic Acid.*

From the way in which the benzoic acid is produced, there is reason to infer that it contains no water of crystallization. From the analysis of it by Berzelius, it seems to weigh exactly 15. The only insoluble salt which this acid forms, as far as I know, is the perbenzoate of iron. I, therefore, endeavoured to verify the supposed weight of this acid in the following way:

15 grs. of benzoic acid were dissolved in distilled water by means of ammonia, and the solution was concentrated till the excess of ammonia was driven off. From the experiments on the protosulphate of iron related in a former paper, it follows that 17·375 grs. of the crystals of protosulphate of iron are composed of

Sulphuric acid	5·0	or 1 atom
Protoxide of iron.........	4·5	1
Water	7·875	7
	17·375	

17·375 grs. of these crystals were dissolved in dilute nitric acid, and the solution was heated till the iron was peroxidized. The liquid was then concentrated as much as possible so as to avoid precipitating the iron, in order to get rid of part of the excess of nitric acid. Ammonia was very cautiously added till the liquid scarcely reddened vegetable blues, taking care not to precipitate any of the iron.

The two solutions thus formed were mixed together. A copious brick red precipitate immediately separated; indeed, it was so abundant (owing to the concentration of the liquids), that it did not speedily subside. The whole was, therefore, thrown upon a filter. The liquid which passed through was transparent and colourless. It was tested for iron and benzoic acid by prussiate of potash and persulphate of iron; but was not affected by either of these reagents. We see from this that 15 grains of benzoic acid are exactly neutralized by five grains of peroxide of iron; consequently an atom of benzoic acid weighs 15, and the crystals of this acid contain no water.

XI. *Succinic Acid.*

This acid being obtained by sublimation, as well as the benzoic, is probably destitute of water; but in its usual state, it is contaminated by a good deal of oil. In the acid which I employed for my experiments, this oil had been removed by digesting it in nitric acid. It was quite white, and crystallized in small four-sided prisms. From Berzelius's experiments, the weight of this acid was probably 6·25.

To verify this opinion, 6·25 grains of the crystals were dissolved in water neutralized by ammonia, and mixed with a solution of 17·375 grs. of protosulphate of iron, peroxidized and neutralized by the very same method which was followed in the case of benzoic acid. The two solutions were mixed together, and the whole was thrown on a filter. The persuccinate of iron remained on the filter in the state of a fine red matter. The liquor which passed through the filter was transparent and colourless, and, when tested with prussiate of potash and persulphate of iron, it gave no indication of containing either iron or succinic acid. From this experiment, it follows that succinic acid weighs 6·25, and that the crystals of it contain no water of crystallization.

Perbenzoate and persuccinate of iron, when dried at the temperature of 212°, contain no water. They furnish, therefore, an easy method of analyzing the benzoates and the succinates; for a benzoate, we have to multiply the weight of perbenzoate of iron obtained by $\frac{15}{20}$, or $\frac{3}{4}$, or 0·75, the product is the weight of benzoic acid; while $\frac{1}{4}$, or 0·25, gives the weight of peroxide of iron; for a succinate, we must multiply the weight of persuccinate of iron by $\frac{6\cdot25}{11\cdot25}$, or 0·555, the product is the weight of succinic acid. If we multiply the weight of persuccinate of iron by

$\frac{5}{11 \cdot 25}$, or by 0·444, we obtain the weight of the peroxide of iron present in the persuccinate.

XII. *Acetic Acid.*

This acid has given me more trouble than any other acid whose atomic weight I have hitherto attempted to ascertain, boracic acid alone excepted. I believe its weight to be 6·25, or exactly the same with that of succinic acid. Probably the crystals of acetic acid are composed of one atom acid + two atoms water, and weigh 8·5; but I have not been able hitherto to verify these conclusions by decisive experiments.

The most careful analysis of acetate of lead which I was capable of making induced me to conclude that it is a compound of

1 atom acetic acid .... = 6·25
1 atom protoxide of lead .... = 14·0
3 atoms water .. .. = 3·375
23·625

Hence I was led to expect that a solution of 23·625 grs. of acetate of lead would be completely decomposed by being mixed with 11 grs. of sulphate of potash, or 9 grs. of anhydrous sulphate of soda; but on trying the experiment, I found that the clear liquid which covered the precipitated sulphate of lead was rendered muddy by the addition of muriate of barytes. Hence it obviously contained sulphuric acid in solution. I repeated this experiment, gradually increasing the quantity of acetate of lead till it amounted to 25 grs. yet the effect still continued; but I found that when nitrate of lead was dropped into the residual liquor, the transparency was not affected. It was clear from this, that acetate of lead does not possess the property of throwing down the whole of the sulphuric acid from the solution of a sulphate. I was of course obliged to abandon this method of experimenting altogether.

Fifty grs. of the crystals of acetate of lead were dissolved in water, and precipitated by carbonate of potash. The precipitate, after being well washed and dried on the sand-bath at a temperature of about 500°, weighed 37·82 grs. It is evident from this, that 23·625 grs. would have yielded 17·51 grs. of carbonate of lead. Now 17·51 grs. of carbonate of lead contain 14·63 grs. of protoxide of lead. This result did not accord with my supposition, that 23·625 grs. of acetate of lead contain exactly 14 grs. of protoxide of lead.

It is not possible, by the most careful experiments which I have tried, to deprive acetate of lead of the whole of its water of crystallization without, at the same time, driving off some of the acid. 23·625 grs. when heated with every precaution, always lost about 3·42 grs. which I consider as a little exceeding the whole water in the salt. Indeed, that it loses acetic acid is obvious from the smell which is emitted from it during the whole time of the application of the heat.

These unsuccessful results induced me to abandon acetate of lead altogether, and to endeavour to determine the weight of acetic acid in quite a different manner.

8·75 grains of carbonate of potash obtained by exposing the bicarbonate to a strong red heat were dissolved in water, and saturated with acetic acid. It is obvious that this portion of carbonate consisted of

	Grains.
Potash	6·00
Carbonic acid	2·75

The acetate was evaporated to dryness, exposed to a heat sufficient to fuse it, and kept for some time in a state of fusion. I was in hopes that at this temperature the salt might be deprived of all its water; but the result did not answer my expectation. I never was able to reduce the weight lower than 14·3 grs.; so that the acetic acid present, had all the water been driven off, would have weighed 8·3 grs. Now I was certain, from many preliminary experiments, which, to avoid tediousness, I have omitted to relate, that the weight of an atom of acetic acid lay between 6 and 6·5. Acetate of potash then cannot be completely freed from water without decomposing the acetic acid, at least partially. When this salt is in fusion, it has a blackish colour; but it becomes white, and assumes a pearly lustre when it becomes solid on cooling.

It is well known that acetate of soda is capable of bearing a higher temperature without undergoing decomposition than acetate of potash. I was in hopes, therefore, that I might succeed in determining the weight of an atom of acetic acid by saturating a given weight of anhydrous carbonate of soda with acetic acid, and exposing the salt to a temperature high enough to drive off all the water without decomposing the salt. This expectation has not been altogether disappointed. Out of at least a dozen of experiments, I have succeeded twice in making the salt perfectly dry without destroying any of the acetic acid; but in the greater number of the trials, the heat was raised so high as to destroy part of the acid. The temperature at which the acid begins to undergo decomposition is pretty well defined. The salt may be heated to about 550° with impunity; but at a temperature which, I think, does not exceed 600°, charcoal is always evolved. My experiments were made on a sand-bath, and if the least inattention allowed the temperature of the sand to get up to 600°, I was sure to find the salt partially decomposed. The experiments were made in four ounce phials. To prevent the liquid from boiling unequally, which endangered the loss of some of it, about 16 grs. of shreds of platinum were put into the phial.

It is obvious that 6·75 grs. anhydrous carbonate of soda are composed of

	Grains.
Soda	4·00
Carbonic acid	2·75
	6·75

Now as 4 represents the weight of an atom of soda, it is obvious that the weight of the acetate of soda obtained by dissolving 6·75 grs. of carbonate of soda in acetic acid, and then drying the salt, diminished by 4, must represent the weight of an atom of acetic acid. Now in the two successful trials which I made, the acetate of soda weighed exactly 10·25 grains; and 10·25 − 4 = 6·25; consequently I consider 6·25 as representing the true weight of an atom of acetic acid.

It may be worth while to relate the attempt which I made to deprive acetate of lime of the whole of its water, though that attempt was unsuccessful. 6·25 grains of calcareous spar were dissolved in acetic acid in a silver crucible, the solution was evaporated to dryness in a very moderate temperature, and the dry salt was left for 12 hours exposed to the open air in the laboratory. The weight of the salt was 11·36 grs. It was now exposed to the temperature of 450° for four hours. By this treatment, the weight was reduced to 10·22 grs.; and no further diminution of weight was produced, though the same temperature was kept up for many hours.

Now it is obvious from the experiments above related with acetate of soda that if the acetate of lime had been completely deprived of its water, its weight would have been 9·75 grs.; for it must have consisted of

Lime	3·5
Acetic acid	6·25
	9·75

The water in the salt when it was gently evaporated to dryness was 1·61 gr. or nearly an atom and a half. I was not able at the temperature of 450° to drive off the whole water; what remained must have weighed 0·47 gr. or rather more than the third of an atom.

The preceding experiments, which I have detailed as briefly as possible, though sufficiently minutely, I trust, to put it in the power of others so inclined to repeat them and verify their accuracy, lead to the conclusion that the different bodies treated of, when in a separate and insulated state, have atomic weights represented by the following numbers:

Bismuth	9·00
Tin	7·25
Antimony	5·5
Mercury	25·0
Arsenious acid	6·75
Boracic acid	2·75
Oxalic acid	9·0
Tartaric acid	9·5
Citric acid	9·5
Benzoic acid	15·00
Succinic acid	6·25
Acetic acid	8·5 ?

Now all these atomic weights (as has been the case with all those determined before) are not only multiples of 0·125, the atomic weight of hydrogen; but of 0·25 = the double of that atomic weight.

Four of these bodies; viz.

Oxalic acid,	Citric acid,
Tartaric acid,	Acetic acid,

appear to contain water of crystallization, of which, however, they cannot be deprived completely without undergoing decomposition. This water, or at least a portion of it, they retain even when combined with bases. Thus when oxalic acid is combined with lime, and merely dried in the open air, or at a temperature of 100°, it retains the half of its water; but it may be deprived of the whole of its water by the action of a stronger heat; yet the acid is not decomposed, for it may afterwards be separated from the lime, and exhibited in its usual crystallized state. Tartaric and citric acid, when they are united to oxide of lead, and the saline compound is dried at the temperature of 100°, are completely deprived of their water. On the contrary, I have been unable to deprive acetic acid of its water, when combined with any other base, except soda.

It would appear from this, that when these acids unite to bases, a certain portion of their water may be separated from them; so that their equivalent number, when they enter into combination, may be represented as follows:

Oxalic acid	4·5
Tartaric acid	8·375
Citric acid	7·25
Acetic acid	6·25 ?

These weights are all multiples of 0·125. They are all multiples of 0·25, except the number for tartaric acid, which evidently contains an odd number of atoms of hydrogen. I entertain some doubts at present whether we should consider the crystals of these acids, or the proportions in which they combine with certain bases as their atomic weight. If we adopt the second plan, we fall into the very extraordinary anomaly, that succinic acid and acetic acid, two acids exceedingly different in their properties, are notwithstanding composed each of the very same constituents. The subject will still require a good deal of research before it can be considered as sufficiently elucidated. Meanwhile it is of importance to draw the attention of chemists to the subject. I shall, therefore, give two tables of the composition of these acids. In the first table, they are supposed to be in the state of crystals; in the second table, they are supposed united to a base, and exposed to a temperature sufficiently high to drive off the whole water which they contain. The second table exhibits these acids as they have been estimated by Berzelius; but I have in some places altered his results somewhat, partly in consequence of the experiments detailed in the present paper, and partly from experiments on their direct decomposition, by heating them along with peroxide of copper; experiments which I have thought it needless to detail, as the results only can be entitled to any attention; and the mode which I followed is now sufficiently understood.

Table I.—*Constituents of Six Vegetable Acids supposed in the State of Crystals.*

Acids.	Hydrogen.	Carbon.	Oxygen.	Total weight.
	Atoms.	Atoms.	Atoms.	
Oxalic	4	2	7	9·0
Tartaric	4	4	6	9·5
Citric.	4	4	6	9·5
Acetic.	4	4	5	8·5
Succinic.	2	4	3	6·25
Benzoic.	6	15	3	15·0

Table II.—*Constituents of Ditto supposed Anhydrous.*

Acids.	Hydrogen.	Carbon.	Oxygen.	Total weight.
	Atoms.	Atoms.	Atoms.	
Oxalic	0	2	3	4·5
Tartaric	3	4	5	8·375
Citric	2	4	4	7·25
Acetic.	2	4	3	6·25
Succinic.	2	4	3	6·25
Benzoic.	6	15	3	15·00

If we adopt the numbers contained in the first table, we make the composition of tartaric and citric acids the same; while the second table renders the composition of acetic and succinic acids identical. Each of these conclusions leads to difficulties which, in the present state of our knowledge, we are unable to explain.

ARTICLE V.

Description of some Minerals found on the newly discovered Antarctic Land. By Thomas Stewart Traill, MD. FRSE. MGS. &c.

(To the Editor of the *Annals of Philosophy.*)

DEAR SIR, *Liverpool, July* 2, 1821.

The return of some of our ships sent to procure seals on the shores of the newly-discovered Antarctic land has furnished me with the following mineral substances, which are chiefly interesting as tending to throw some light on the geology of that dreary and desolate region.

1. *Trap Rock.*—This, in some specimens, is an amygdaloidal greenstone, containing distinct grains of hornblende. It greatly resembles the rock in which the zeolites of the Ferroe Isles are chiefly found. In one specimen, now before me, it seems passing into basalt; and, when traversed by veins of quartz and chalcedony, it becomes extremely hard, resisting the edge of the knife, shows a blackish colour, and a conchoidal fracture, and is perfectly similar to the rock of Portrush, in Ireland, in which the ammonites are found. This latter rock I examined about two years ago, and found it to be a bed between two layers of common greenstone, with which, in some places, it is intimately blended.

2. *Apophyllite in fine Crystals.*—These are low rectangular prisms, with the solid angles truncated, or replaced by triangular planes. Some of the largest crystals, in my specimen, measure more than 3-10ths of an inch in length, and 2-10ths in breadth. The specimen forms a group upon rhombs of calc-spar, and bears so striking a resemblance to a specimen of apophyllite from Ferroe, that the eye cannot detect the difference between them. The apophyllite of New South Shetland exfoliates readily in the flame of a common candle, it breaks down into flakes in nitric acid (though not quite so readily as a specimen from Ferroe), and at length forms a jelly. The pearly lustre is confined to the terminal planes of the crystals, while that of the sides is vitreous. These characters are sufficient to distinguish the apophyllite from a few crystals of stilbite, which I found adhering to the mass. Though none of the matrix accompanies my specimen, the trap is most probably the rock in which this mineral occurs.

3. *Stelbite.*—A few crystals of this substance are mixed with the apophyllite, and also shoot among quartz crystals in drusy cavities.

4. *Druses, chiefly containing Quartz Crystals, with a few Crystals of Apophyllite and Zeolite occasionally intermixed.*—

These are remarkable for their perfect similarity to some of the druses found in the amygdaloid of Ferroe. The cavity has first a thin lining of chalcedony, not above 1-30th of an inch in thickness, yet pretty uniformly spread over the irregular surface of the cavity in the matrix: then appears a coat of an opaque snow-white substance, hard, and brittle, which easily scratches glass, and is not melted by the common blowpipe, nor acted on by nitric acid. It passes by imperceptible shades into pyramids of transparent quartz. On comparing this specimen with one brought by my friend Major Petersen, from Ferroe, and another from Kiose Syssel, in Iceland, the similarity of their structure was such, that they might have passed as fragments of the same specimen.

5. *Chalcedony* in small veins, and in druses.

6. *Calc Spar*, both massive and in rhombic crystals, which appear to have lined cavities in the trap rock.

7. *Iron Pyrites* disseminated in minute grains in the latter.

When we add to these the well authenticated occurrence of coal in considerable quantity in that part of New South Shetland from which my specimens were brought, we must consider that region as belonging to a floetz trap formation; and we cannot fail to remark the strong resemblance between the geological features of the new Antarctic land, and some of the countries near, and within the Arctic circle. Should it be afterwards found, as is highly probable, that New South Shetland consists of a cluster of large islands; the analogy of this group to the land around Baffin's Bay will connect in geographical relation the two extremes of our planet.

The existence of unchanged bones of different cetaceous animals, and of seals, on the top of the mountains in New South Shetland is fully confirmed; and there is now in my possession the skull of an animal belonging to the class Mammalia, found on the top of a considerable mountain in that country, which, from a hasty inspection, appears to have belonged to a large species of seal. The quantity of those comparatively recent organic remains, which are said to occur in New South Shetland, and the want of inhabitants on its inhospitable shores, leave us no more plausible conjecture to account for their present extraordinary situation, than that the hills where they occur have, at no very distant period, been suddenly elevated from the bosom of the deep by some vast convulsion, most probably the effect of subterranean fire. Should this be the case, it will tend to confirm the extensive agency of volcanic fire in moulding the surface of our globe, which the invaluable researches of the illustrious Humboldt, among the Cordilleras of both Americas, and the deductions of Von Buch, have rendered highly probable. Indeed the candid geologist must acknowledge, that some of our most consistent and celebrated speculators on the theory of the earth have not sufficiently estimated the extensive agency of volcanic fire in contributing to the present arrangement of the surface of our globe; and a diligent observer must admit the strong points of resemblance between the floetz trap formations, and the undoubted products of volcanoes.

I am, dear Sir,
Your most obedient servant,
Thomas Stewart Traill.

Article VI.

On Carburet of Nickel. By Mr. Irving.

(To the Editor of the *Annals of Philosophy.*)

Sir, *Birmingham, July* 10, 1821.

I read with some degree of interest the communication respecting carburet of nickel, contained in the last number of your *Annals*.

I have been in the possession of the article for several years, and I have at different times made considerable quantities of it. I find that it may be very readily formed by the agency of very strong heat. I should long since have sent a statement of the discovery to some of the periodical publications, and thus have secured to myself the priority, in reference, at least, to your correspondent Mr. Ross, had I not been restrained by the remark in Dr. Thomson's System, that by forming nickel after the manner he has described, "it always contains a portion of carbon." I imagined that no one who had been apprized of this fact could have had any doubt of the formation of the carburet, when obvious means were had recourse to for the purpose.

I write to you at present in haste, or I should have begged to state more particulars; and though I regard the subject as of trifling importance, I think it due to myself to request that you will allow me to state, in your publication, my claim to priority of discovery. R. Irving.

Article VII.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

The following papers have been read since our last report:

July 5.—On the Magnetic Phenomena produced by Electri-

city and their Relation to Heat occasioned by the same Agent, by Sir H. Davy.

July 12.—An Investigation of some Theorems relating to the Theory of the Earth, &c. by M. Wronski.

On the Peculiarities of the Manatee of the West Indies, by Sir E. Home.

On a new Compound of Chlorine and Carbon, by Messrs. R. Phillips and M. Faraday.

This compound was brought to England, and given to these gentlemen by M. Julin, of Abo, in Finland, having been formed during the distillation of green vitriol and nitre for the production of nitric acid. It is a solid crystalline body, fusible and volatile by heat without decomposition, and condensing into crystals. It is insoluble in water, but soluble in alcohol, ether, and essential oils. It sinks in water. It burns with a red flame, giving off much smoke, and fumes of muriatic acid gas. Acids do not act on it. When its vapour is highly heated in a tube, decomposition takes place, chlorine is given off, and charcoal deposited. Potassium burnt with it forms chloride of potassium, and liberates charcoal. Its vapour, detonated with oxygen over mercury, formed carbonic acid and chloride of mercury;—passed over hot oxide of copper, it formed a chloride of copper and carbonic acid; and, over hot lime, it occasioned ignition, and produced chloride of calcium and carbonic acid. It is composed of chlorine and carbon; and from the experiments detailed, two parts appear to be formed, of

1 portion of chlorine	44·1	33·5
2 portions of carbon......	15·0	11·4

Hence it is a subchloride of carbon. All attempts to form it by other means have hitherto failed.

On the Structure and Functions of the Nerves, by Charles Bell, Esq.

The Society then adjourned to the usual period.

GEOLOGICAL SOCIETY.

May 4.—The continuation of Mr. Strangway's paper, on the Geology of Russia, was read.

The central salt district occupies an extensive tract of country, which is partly marked by the course of the Volga, but will scarcely admit of any precise geographical boundary. In the governments of Yaroslaf, Costrenia, and Vologua, salt is made from brine; and near the Volga, at Balakhna, where there are several brine springs situated in a plain between the hills and the river, one spring, opened in 1818, affords 13° of salt. The general character of the rocks in this district is red sandstone passing into marl, and the soil is, for the most part, very fertile.

When ravines occur, as on the banks of the Oca and Volga, near Nishroy Novgorod, they exhibit only horizontal strata of red and white marl, with occasional alternations of red sandstone.

Near Pechersk, the colour of this sandstone varies from a pale-red to a dusky-green, and is marked by irregular concretions, and a sort of globular structure like that of Borovichy and the Pepovca. At the bottom of this cliff, along the shore, it is a thick bed of a very compact tufa, of which the cavities are often filled with brown sulphate of lime disposed stalactitically, and capable of taking a fine polish. Freshwater shells are sometimes found in this tufa. A similar formation exists in the Oca, near the salt magazine.

Among the most remarkable natural curiosities of the government of Nishroy Novgorod are the rocks and cavern of Barnoucova, which are situated near the western extremity of a ridge of hills that bound the northern bank of the river Piana, and are distinguished by their romantic beauty and resplendent purity of the alabaster of which they are composed. It is indistinctly stratified in large beds, and contains starry crystallizations of selenite. The neighbouring country is wholly of red rock marl, in which the alabaster forms subordinate beds.

At the village of Troitska, 11 versts further up the Piana, a fibrous gypsum, which has been mistaken for the mineral called rock leather, is found in continuous layers between the beds of marl, and may be pulled out with care in sheets of several square feet.

At Simberck, the upper part of the hills in the Volga contains great quantities of a very white marl (*kreide-mergel*); and the limestone of Cazan appears to belong to the same formation. It is of a greyish colour, usually very distinctly oolitic, and at the same time much harder and more compact than the rocks which form the oolitic series in England. At the former place also a black clay containing pyrites and green sand, and usually full of organic remains, is found. It appears also at Polymnia, a little higher up the river; at Mourzikha, on the Soura; at Vixa, and in some parts of the neighbourhood of Moscow. The apparent situation of this rock is beneath the sand.

At Tatuski, 24 versts west of Moscow, a pinkish-white siliceous sandstone is found in large slabs with irregularly curved surfaces. It is used for foundations and for millstones.

ARTICLE VIII.

SCIENTIFIC INTELLIGENCE, AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Third Report of the Commissioners appointed by His Majesty to consider the Subject of Weights and Measures.*

May it please your Majesty,

We, the commissioners appointed by your Majesty, for the purpose of considering the subject of weights and measures, have now completed the examination of the standards which we have thought it necessary to compare. The measurements which we have lately performed upon the apparatus employed by the late Sir George Shuckburgh Evelyn, have enabled us to determine, with sufficient precision, the weight of a given bulk of water, with a view to the fixing the magnitude of the standard of weight; that of length being already determined by the experiments related in our former Reports: and we have found by the computations, which will be detailed in the Appendix, that the weight of a cubic inch of distilled water, at 62° of Fahrenheit, is 252·72 grs. of the Parliamentary standard pound of 1758, supposing it to be weighed in a vacuum.

We beg leave, therefore, finally to recommend with all humility, to your Majesty, the adoption of the regulations and modifications suggested in our former Reports; which are principally these:

1. That the parliamentary standard yard, made by Bird in 1760, be henceforwards considered as the authentic legal standard of the British empire; and that it be identified by declaring, that 39·1393 inches of this standard, at the temperature of 62° of Fahrenheit, have been found equal to the length of a pendulum supposed to vibrate seconds in London, on the level of the sea, and in a vacuum.

2. That the parliamentary standard troy pound, according to the two pound weight made in 1758, remain unaltered; and that 7000 troy grs. be declared to constitute an avoirdupois pound; the cubic inch of distilled water being found to weigh at 62°, in a vacuum, 252·72 parliamentary grains.

8. That the ale and corn gallon be restored to their original equality, by taking for the statutable common gallon of the British empire, a mean value, such that a gallon of common water may weigh 10 pounds avoirdupois in ordinary circumstances, its content being nearly 277·3 cubic inches; and that correct standards of this imperial gallon, and of the bushel, peck, quart, and pint, derived from it, and of their parts, be procured without delay for the Exchequer, and for such other offices in your Majesty's dominions, as may be judged most convenient for the ready use of your Majesty's subjects.

4. Whether any further legislative enactments are required, for enforcing a uniformity of practice throughout the British empire, we do not feel ourselves competent to determine. But it appears to us, that nothing would be more conducive to the attainment of this end than to increase, as far as possible, the facility of a ready recurrence to the

legal standards, which we apprehend to be in a great measure attainable by the means that we have recommended; it would also, in all probability, be of advantage to give a greater degree of publicity to the Appendix of our last Report, containing a comparison of the customary measures employed throughout the country.

5. We are not aware that any further services remain for us to perform in the execution of the commands laid upon us by your Majesty's commission; but if any superintendence of the regulations to be adopted were thought necessary, we should still be ready to undertake such inspections and examinations as might be required for the complete attainment of the objects in question.

(Signed) George Clerk.
London, Davies Gilbert.
March 31, 1821. W. H. Wollaston.
Thomas Young.
Henry Kater.

APPENDIX.

The commissioners having been furnished, by the kindness of the Hon. Charles C. C. Jenkinson, with the apparatus employed by the late Sir George Shuckburgh Evelyn, in the determination of the magnitude of the standard weights, and there being some doubt of the perfect accuracy of his method of measuring the capacity of the bodies employed, it was judged necessary to repeat that measurement with greater precautions; and the results of Capt. Kater's experiments have afforded some slight corrections of the capacities in question.

The sides of Sir George Shuckburgh's cube were found by Captain Kater equal to 4·98911, 4·98934, and 4·98935 inches; the diameter of the cylinder 3·99713, and its length 5·99600 inches; and the diameter of the sphere 6·00759 inches. Hence the content of the cube appears to be 124·1969 inches; that of the cylinder 75·2398; and that of the sphere 113·5264 inches of Bird's parliamentary standard of 1760, recommended in the last Report of the commissioners, or of the standard made by Troughton for Sir George Shuckburgh.

The difference of the weight of the cube in air at 62°, with the barometer at 29·0, and in water at 60·2°, was 31381·79 grs.; and adding to this the weight of an equal bulk of the air at 62°, which is $\frac{1}{834\cdot\frac{29}{30}}$ of that of the water, or 36·26 grs. and subtracting from it $\frac{1}{8\cdot5}$ of this, or 4·26 grs. the buoyancy of the brass weights, we obtain 31413·79 grs. for the weight of the cube of water in a vacuum at 60·2°. Now this cube is less than the supposed measure at the standard temperature of 62°, in the ratio of 1 to 1·0000567, on account of the contraction of the brass; and the water is denser than at the standard temperature, according to Mr. Gilpin's experiments, in the ratio of ·99998 to ·99981, or of 1·00017 to 1, the whole correction, for the difference of 1·8°, being ·0001133, or 3·55 grains, making 31410·24 for the weight of the cube of water in a vacuum at 62°; which, divided by 124·1969, gives 252·907 for the weight of a cubic inch in Sir George Shuckburgh's grains.

In the same manner, we obtain for the cylinder, which was weighed in air under the same circumstances, and in water at 60·5°, the difference being 19006·83 grs. the correction $\frac{1}{834\cdot\frac{29}{30}\cdot\frac{7\cdot5}{8\cdot5}}$ for the effect of buoyancy, amounting to 19·43 grs. and for the difference of temperature of the water and brass conjointly, the densities being ·999955 and ·999810, the correction ·000145 — ·000047 = ·000098, or 1·86 grs. leaving + 17·57 grs. for the whole correction of the weight, as reduced to a vacuum at 62°, and making it 19024·40, which, divided by 75·2398, the content of the cylinder, affords us 252·851, for the cubic inch in a vacuum at 62°.

The sphere was weighed in air at 67°, the barometer standing at 29·74; the correction for buoyancy is here $\frac{75}{85}\cdot\frac{29\cdot74}{30}\cdot\frac{1}{843}$, or for 28673·51 grs. 29·72: while the temperature of 66° requires, for the difference between the expansion of brass and water, the addition of ·00042 — ·000126, or ·000294 of the whole; that is + 8·43 grs. making the whole correction 38·15, and the weight in a vacuum 28711·66; which, divided by 113·5264, gives us 252·907, for the cubic inch in a vacuum.

The mean of these three measures is 252·888, giving for the three errors + ·019, — ·037, and + ·019; and this mean, reduced to the parliamentary standard, makes 252·722 grs. for the cubic inch of distilled water at 62°, weighed in a vacuum, or 252·456 in air, under the common circumstances of the atmosphere, when weights of brass are employed. In a vacuum at the maximum of density, that is at 39°, the weight of a true cubic inch will be 253 grs. and of a cubic decimetre 15440.* The proposed Imperial Gallon, of 10 pounds, or 70000 grs. of water, will contain very nearly 277·3 cubic inches under common circumstances.

II. *Potash from Potatoe Stalks.*

According to some experiments stated to have been made in France, and detailed in the Philosophical Magazine for November, 1817, 2000 lb. of potash are obtainable from the stalks of an acre of potatoes. Mr. Rice, who tried the experiment in Ireland, found that only 201½ lb. could be obtained. With a view to verify or correct the French statement, Sir John Hay, Bart. at the request of Dr. Macculloch, made a large experiment on his farm near Peebles. Dr. Macculloch states, that the experiment was conducted implicitly, according to the directions given in the narrative of the French experiment, from the cutting to the burning of the plant; and the ashes were examined by Dr. Macculloch himself.

The results of two trials on separate acres were as follow: premising that the Scotch acre is one-seventh larger than the English, and presuming that, in the original statement, the measures were reduced to the English acre. The first acre was a rich loamy soil at King's Meadows; the potatoes were drilled, and produced a good crop. They were cut, as directed, immediately after flowering, left 10 days to dry, and burnt in a pit. The produce was 222 lb. of ashes, and in lixiviation and drying these yielded 55 lb. of impure potash, or mixed salts.

The second acre was a clayey wet soil, with a retentive bottom; but the crop, which was also drilled, was considered moderate. These stems were treated in the same manner; but the burning was more complete, the ashes containing less charcoal than the preceding. They weighed only 112 lb. and produced 28 lb. of impure potash.

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* It appears, however, from an official Report, obligingly communicated to us by Dr. Kelly, that the actual standard chiliogramme has been found to contain only 15438 English grains.

Taking a mean result from the experiments made in Ireland and Scotland, or even admitting the former to afford a better standard, there is evidently no temptation, says Dr. Macculloch, for agriculturists to repeat these trials with a view to profit; for, on analysis, the aline mass called impure potash did not contain above 10 per cent. of pure alkali.—(Journal of Science.)

III. *Chromic and Sulphuric Acids.*

When sulphuric acid is boiled on chromate of lead or barytes in excess, the chromic acid obtained is not pure, but contains sulphuric acid. The liquid containing the two acids, when successively evaporated, entirely crystallizes in small quadrangular prisms of a deep red colour. If the heat and concentration be carried too far, oxygen is disengaged, and sulphate of green oxide of chromium found. These crystals are deliquescent, and contain one atom of each of the acids. To analyze them, they were boiled with a mixture of muriatic acid and alcohol, so as to convert the chromic acid into green oxide; then, dividing the liquid into two parts, one was precipitated by muriate of barytes, to give the sulphuric acid; and the other by ammonia, for the oxide of chrome, and, consequently, the chromic acid.

Alcohol easily dissolves this substance, and, if strong, so rapid a decomposition is produced, as to resemble an explosion. The chromic acid becomes oxide of chromium, and a particular ethereal odour is produced. Having ascertained that the same odour was produced by treating peroxide of manganese with alcohol and sulphuric acid, I collected some of this ethereal fluid by distillation, and rectified it on lime to separate water, and on chloride of calcium to separate alcohol. It was then of an acrid burning taste, and very penetrating odour, resembling sulphuric ether. When mixed with water, it separated into a stratum of sulphuric ether, and a white transparent light oil, identical with the sweet oil of wine. The mixture of alcohol, sulphuric acid, and black oxide of manganese, that had been used, contained much sulphate of manganese, but no hyposulphuric acid.

Hence, in treating alcohol by chromic and sulphuric acid, or by the latter and peroxide of manganese, it appears to undergo the same alteration as by sulphuric acid alone. Sulphuric ether and sweet oil of wine are formed by means of the oxygen of the chromic acid, or of the peroxide of manganese. The sulphuric acid suffers no alteration, but its presence is necessary to determine the decomposition of the alcohol and the partial deoxidation of the chromic acid, or peroxide, in consequence of its affinity for the oxides of chromium and manganese. I do not doubt but that it might be replaced by many other acids.

M. Gay-Lussac, to whom these experiments are due, then observes, that Scheele and Dobereiner had noticed effects relative to this subject. Scheele remarked the ethereal smell, &c. produced by the action of peroxide of manganese, sulphuric acid, and alcohol, and distilling slowly; and Dobereiner had observed a similar odour in a mixture of chromate of potash, sulphuric acid, and alcohol.—(Ann. de Chim. vol. xvi. p. 103.)

IV. *Process of analyzing Gunpowder.*

The process usually employed for analyzing gunpowder consists in washing the powder with water in order to separate the nitre, and treating the residuum with potash, which dissolves the sulphur and leaves the charcoal. Although this process appears to be easy, it is attended with some difficulties, which are discoverable only in the execution of it; the use of this method must not, however, be condemned; and is necessary to have recourse to it, if the quantity of charcoal contained in the powder is to be determined in a direct mode. When it is requisite to use this method, it is better to take two portions of powder; one of them is to be washed to obtain the nitre; the residuum is to be dried and weighed. The other portion is to be mixed with an equal quantity of potash and a little water, and the mixture is to be heated: the sulphur readily dissolves, and it is then to be washed until the water has no sulphurous smell, or until it does not precipitate acetate of lead of a black colour. The charcoal is to be dried and weighed. The weight of the sulphur is to be estimated by deducting that of the nitre and charcoal from the weight of the dry gunpowder employed. The results of the analysis may be verified by comparing the weight of the sulphur and charcoal left by the first portion of powder with that given by the second.

In following this process, there is to me uncertainty in determining the proportion of charcoal, and which exists, also, with respect to the quantity of sulphur. If, therefore, the weight of the sulphur could be determined by direct means, the analysis of the powder would be much more exact. For the purpose of accomplishing this, the following process is described; and its correctness has been proved by a great number of experiments.

A certain quantity of powder is to be dried in order to determine the proportion of moisture which it contains, and to determine with certainty the quantity of charcoal which, in this process, is obtained only by subtraction. The nitre is estimated by washing the powder, evaporating the washings, and fusing the saline residuum.

In order to determine the quantity of sulphur, 75 grs. of the powder, and an equal weight of pure subcarbonate of potash, are to be mixed. The mixture is to be well pulverized in a mortar, and 75 grs. of nitre and 300 of common salt are then to be added. These, after having been thoroughly mixed, are to be heated in a platina vessel in a charcoal fire; the combustion of the sulphur takes place slowly, and the mass soon becomes white. The operation is then finished: the vessel is to be removed from the fire, and, when cold, the saline mass is to be dissolved in water, the solution is to be saturated with nitric or muriatic acid, and the sulphuric acid precipitated by muriate of barytes.

There are two modes of effecting this precipitation: the first, which is generally followed, consists in adding a slight excess of muriate of barytes, and collecting the sulphate of barytes produced. This process requires numerous washings, which can only be made at long intervals, because sulphate of barytes subsides slowly, especially towards the end of the operation, at which time this salt often remains suspended, and goes through the thickest filters. If the sulphate of barytes be washed upon the filter, fresh inconvenience arises; the sulphate must be detached from the filter, or they must be weighed together; and, in either case, an error is easily made, especially if the operator is not well skilled.

The other mode of precipitating the sulphuric acid, and which it is here proposed to adopt, is to take a solution of muriate of barytes of known strength, and to pour this solution into that which contains the sulphuric acid, until precipitation ceases. When the precipitation is nearly complete, the solution of muriate of barytes should be added only by drops. The solution, after each addition of the muriate of barytes, is to remain till it becomes clear before any more is added.

To expedite the process, a portion of the liquor may be filtered, and a drop of muriate of barytes added to it. The same filter will serve for the whole operation. There is no danger of the sulphate of barytes passing the filter in this operation; this only occurs when the water contains but little or no saline matter in solution.

The quantity of sulphuric acid, and, consequently, that of the sulphur, is given by the weight of the muriate of barytes employed; the equivalent number for sulphur being 20·116, and that of crystallized muriate of barytes 152·44. It will be sufficient to make this proportion: 152·14 : 20·116 :: the quantity of muriate of barytes employed is to a fourth term, which is the quantity of sulphur sought.

This process, it appears, is used at the laboratory of the powder manufacturers in France, and it is stated to determine the quantity of sulphur within 1-500th or 1-1000th part. The common salt is used to moderate the deflagration; and the carbonate of potash is necessary to prevent the sulphuric acid from being volatilized.—(Ann. de Chim. et Phys.)

Upon this process, I would merely remark, that there is no danger of sulphate of barytes passing through the filter, if the solution from which it is precipitated be hot; and it very readily subsides in hot water. I have not found any difficulty in ascertaining the quantity of sulphate of barytes, provided a double filter be used; the outer one to serve, of course, as a counterpoise to that which contains the sulphate of barytes.—*Ed.*

ARTICLE IX.

NEW SCIENTIFIC BOOKS

PREPARING FOR PUBLICATION.

Lectures on Botany, by Anthony Todd Thomson, Esq. FLS.

Dr. Conquest will publish in a few weeks, a Second and enlarged Edition of his "Outlines of Midwifery, &c." with Copper-plate instead of Lithographic Engravings.

JUST PUBLISHED.

A Treatise on the Nature and Treatment of Scrophula, describing its Connexion with Diseases of the Spine, Joints, Eyes, Glands, &c. founded on an Essay to which the Jacksonian Prize, for the Year 1818, was adjudged by the Royal College of Surgeons. To which is added, a brief Account of the Ophthalmia, so long prevalent in Christ's Hospital. By Eusebius Arthur Lloyd. 8vo. 9*s*.

A Treatise on Acu-puncturation, a Description of a Chinese Surgical Operation now introduced into European Practice. By James Morss Churchill. 8vo. 4*s*.

Observations on certain Affections of the Head, commonly called Headaches, with a View to their more complete Elucidation, Prevention, and Cure; together with some brief Remarks on Digestion and Indigestion. By James Farmer, Member of the Royal College of Surgeons in London. 18mo. 2*s*.

Practical Observations on Cold and Warm Bathing; and descriptive Notices of Watering Places in Britain. By James Miller, MD. Fellow of the Royal College of Physicians, and Lecturer on Natural History and Chemistry. 12mo. 4*s*. 6*d*. boards.

Researches into the Laws and Phenomena of Pestilence; including a Medical Sketch and Review of the Plague in London in 1665, and Remarks on Quarantine. By Thomas Hancock, MD. 8*s*.

New Descriptive Catalogue of Minerals; with Diagrams of their simple Forms: intended for the Use of Students, in the Classification of Minerals, and the Arrangement of Collections. By John Mawe. The Fourth Edition, entirely re-written, and considerably enlarged. 7*s*.

One Thousand Experiments in Chemistry; with Illustrations of Natural Phenomena; and Practical Observations on the Manufacturing and Chemical Processes at present pursued in the successful Cultivation of the Useful Arts, with numerous Engravings on Wood and Copper. By Colin Mackenzie. 8vo. 1*l*. 1*s*.

Scientific Amusements in Philosophy and Mathematics; together with amusing Secrets in various Branches of Science. By W. Enfield, MA. 12mo. 3*s*. 6*d*.

An Account of the Interior of Ceylon and of its Inhabitants, with Travels in that Island. By John Davy, MD. FRS. 4to. 3*l*. 13*s*. 6*d*.

Zoological Researches in the Island of Java, &c. &c. with Figures of Native Quadrupeds and Birds. By Thomas Horsfield, MD. To be comprised in Eight Numbers. No. I. royal 4to. 1*l*. 1*s*.

Article X.

NEW PATENTS.

Aaron Manby, of Horseley, near Sipton, Staffordshire, iron master, for improvements in manufacturing steam-engines.—May 9, 1821.

Samuel Hall, of Basford, Notts, cotton-spinner, for an improvement in the manufacture of starch.—May 9.

George Frederick Eckstein, of High Holborn, ironmonger, for certain improvements in cooking apparatus.—May 9.

John Mayor, of Strawbury, and Robert Cook, of Shrewsbury, Salop, for certain improvements in machinery for raising water, which is to be denominated Hydragogue.—May 9.

Robert Paul, of Starton, Norfolk, and Samuel Hart, of Redenhall-with-Harleston, Norfolk, for an improvement in springs for carriages. —May 17.

ARTICLE XI.

METEOROLOGICAL TABLE.

1821.	Wind.	Barometer. Max.	Barometer. Min.	Thermometer. Max.	Thermometer. Min.	Evap.	Rain.	Hygr. at 9 a.m.	
6th Mon.									
June 1	E	30·09	30·04	74	43	—			
2	N E	30·04	29·95	76	44	—			
3	N E	29·95	29·75	75	47	42	03		
4	N W	29·77	29·67	67	45	—	03		
5	N W	29·89	29·77	74	52	—	34		
6	N W	29·93	29·83	69	48	33	11		
7	S W	29·83	29·69	69	50	—	1·08		☽
8	N E	29·86	29·69	56	36	—			
9	N W	29·87	29·80	68	40	—	12		
10	N W	29·93	29·80	60	35	—	10		
11	N E	30·24	29·93	57	43	40	08		
12	Var.	30·28	30·24	57	41	—			
13	N W	30·31	30·28	59	36	—			
14	N E	30·34	30·31	65	40	—			
15	N E	30·34	30·25	69	44	—			●
16	N E	30·31	30·25	65	48	50	01		
17	N E	30·33	30·31	61	48	—			
18	N E	30·31	30·24	67	43	—		75	
19	N E	30·24	30·14	65	40	47	01	66	
20	N	30·14	30·12	65	37	—		72	
21	N E	30·19	30·12	66	34	—		66	
22	N	30·20	30·19	61	48	—		64	☾
23	N	30·20	30·16	61	38	—		67	
24	N E	30·16	30·15	66	44	47		68	
25	N E	30·16	30·15	71	49	—		69	
26	N E	30·15	30·12	69	49	—		70	
27	N E	30·19	30·12	68	45	45		64	
28	N E	30·19	30·16	74	40	—		65	
29	Var.	30·16	30·02	77	46	—		61	○
30	S W	30·02	29·65	81	54	43	31	62	
		30·34	29·65	81	34	3·47	2·22	75—61	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

REMARKS.

Sixth Month.—1, 2, 3. Fine. 4, 5. Cloudy. 6. Cloudy: showers. 7. A very heavy shower of rain and hail from three to four o'clock, p. m. 8. Cloudy. 9. Cloudy: rainy night: some hail at half-past nine, a. m. 10, 11. Showery. 12. Cloudy. 13. Cloudy and fine. 14. Cloudy: fine. 15. Ditto. 16, 17. Ditto. 18, 19, Fine. 20, 21, 22. Overcast. 23. Fine: overcast. 24, 25. Overcast. 26. Fine: cloudy: very fine *Cirrus* in the morning. 27. Fine: clear. 28, 29. Fine: *Strati* in the marshes at night.

RESULTS.

Winds: N, 3; NE, 16; E, 1; SW, 2; NW, 6; Var. 2.

Barometer: Mean height	
For the month	30·075 inches.
For the lunar period, ending the 22d	30·062
For 13 days, ending the 7th (moon north)	29·969
For 15 days, ending the 22d (moon south)	30·152
Thermometer: Mean height	
For the month	55·316°
For the lunar period	52·758
For 31 days, the sun in Gemini	51·661
Evaporation	3·47 in.
Rain	2·22
Mean of hygrometer for 13 days	66°

Laboratory, Stratford, Seventh Month, 21, 1821. R. HOWARD.

ANNALS

OF

PHILOSOPHY.

SEPTEMBER, 1821.

ARTICLE I.

On the Geology of the Neighbourhood of Okehampton, Devon. By the Rev. J. J. Conybeare.

(To the Editor of the *Annals of Philosophy.*)

DEAR SIR,

THE western branch of the river Okement has, from the general beauty of its scenery, and the singularly picturesque site of the castle, which guards the entrance of the defile through which it flows, long ago attracted the notice of tourists. Its course, like that of most mountain streams, affords also to the geologist a tolerably perfect section of the rocks superposed upon that from which it takes its rise (in this case, the central granite of Dartmoor). His researches will be facilitated by the magnificent road lately cut on its NW bank by Mr. A. Saville, and by the extensive limeworks carried on at Meldon Quarries. I have to regret that my own inspection was too cursory, and my collection of specimens too limited, to afford materials for a fuller and more accurate description; but as none has, I believe, hitherto been made public, the following memoranda may not, perhaps, be altogether uninteresting.

Believe me, with much esteem, dear Sir,
Very truly yours,
J. J. CONYBEARE.

The *granite* through which the Okement first flows as a mountain torrent presents the same general character with that of the whole Dartmoor range. It is white, and coarse-grained. It contains occasionally veins of felspar, quartz, and tourmalin, and exhibits, though sparingly, the remarkable contemporaneous patches of a finer grain, which have been noticed in other places, and might at first sight be mistaken for imbedded masses. In one of these, I observed an intermixture of extremely minute portions of tourmalin, and a tendency to orbicular aggregation, which, had the rock been less friable, and its formation more perfect, might have afforded specimens for the lapidary. In another block, I noticed a remarkable disposition of quartz and felspar in alternate layers, of about one-fourth of an inch broad, each retaining their usual semicrystalline aspect. This variety evidently passed into the common form of granite. The laminated portions (if I may so call them) contained little or no mica. The immediate junction of the granite with the superincumbent rock is concealed on the banks of the river by vegetation, and in its bed by the accumulation of bowlders. Among these, however, are found portions of the same obscure gneiss, formed apparently by the union of a dark violet coloured slaty felspar, with a small portion of mica (or chlorit?) and quartz, which occurs in many parts of the Ocrynian groupe, immediately reposing on granite.* It is probable, therefore, that this is the case in the course either of the Okement, or of one of its tributary streams. The first rock, however, which can be traced *in situ* (A) is a hard compact black slate, not very readily fissile, yet exhibiting in parts a decidedly laminated structure. Its fracture in the more massive varieties is imperfectly conchoidal, and the fragments thick, under the hammer it is rather tough than brittle, and, before the blowpipe, fuses reluctantly into a nearly opaque glass, of a muddy-white tinged with green. A high magnifying power shows minute brilliant specks, apparently of mica, disseminated through its mass. It is rather crushed than abraded by a common file, and acts as a good touchstone for copper and silver. I have been thus particular in describing this rock, because I am somewhat uncertain whether to ascribe it to the greenstone series which follows, or to consider it (which I am rather disposed to do) as an indurated and massive variety of the clay slate, or killas, which usually reposes on the granite of the west.† This rock is traversed in every direction by granitic and felspathic veins, varying in breadth from some feet to a mere line, and exhibiting frequently even in hand specimens very singularly tortuous forms.* In one spot this rock has been excavated under the hope of finding tin. The trial appears to have been unsuccessful. The *Deads* afforded specimens of coarse dark-brown garnet, associated with calcareous spar and axinite, the latter much disguised by its incorporation with the rock which serves as its matrix, and which, as well as its imbedded minerals, is much charged with oxide of iron, and considerably increased in weight. It is also rendered, perhaps by these extraneous admixtures, much more fusible.† This rock is succeeded by thick and nearly vertical beds of a very compact greenstone (B), exhibiting for the most part rather the character of granular felspar tinged by the intimate admixture of hornblende. The face of Meldon Hill formed by this rock (on the left bank of the stream) is precipitous and highly picturesque. Here the felspathic veins appeared to cease, but the garnet and axinite still occurred in small contemporaneous veins, and were somewhat more distinctly characterized than in the slate A. We found traces also of epidote. Upon this greenstone rests, at a very high angle, the limestone (C) of Meldon Quarry interstratified with beds of (hornblende?) slate more or less compact, and occasionally of granular felspar nearly free (as far as the eye can judge) from any foreign admixture. When the alternation of this with the dark hornblende slate has taken place on a small scale, it affords very handsome specimens for the cabinet.‡ The limestone itself is black, of an earthy texture, good quality, and extensively worked. It contains, as far as our observation went, no traces of organic remains.§ In the beds of granular felspar, we observed minute rifts coated with a mixture of calcareous spar and a mineral of a light-brown colour, and considerable lustre, beautifully arranged in stelliform groupes composed of numerous minute prisms radiating from a common centre. If it be not a variety of epidote (to which species, from its comparative hardness and ready fusibility before the blowpipe, I am disposed to refer it) it is possibly among the substances hitherto undescribed.* Associated with this series of rocks, we found a variety of compact felspar so penetrated partly by hornblende, and partly by the carbonaceous matter which tinges the limestone (C) as to assume a deep-black colour.† This is distinguished from the compact slate A by its more conchoidal fracture, its greater brittleness and fusibility before the blowpipe.

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* See Prof. Sedgwick's Memoir on Cornwall (Cambridge Phil. Trans. p. 112, 113). At the southern extremity of Dartmoor, I have noticed this rock near Ivy Bridge, at Buckland in the Moor, and especially in the channel of a small stream which flows into Holme Chase a little above the road to Spitchwick. It is found, traversed by small veins of felspar and tourmalin, and forming from its colour and perfection, handsome and well characterized specimens, at a mine sunk upon the point of junction at Kith Hill, near Callington. I have ventured to use the term Ocrynian on the authority of Messrs. Greenough and W. D. Conybeare.

† I should refer it to Division I, B. 6, Argil. Schist of Dr. Macculloch's Classification, p. 355

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* I subjoin a list of some among the more obvious: their general resemblance to the Elvans of the Cornish Killas will be immediately seen.

1. Granular felspar and quartz, with some traces of silvery mica.
2. Same aggregate, more crystalline, and with a greater portion of mica.
3. Same, with numerous plates of silvery mica (mica talcite?) and specks of dark violet coloured fluor spar.
4. Quartz and tourmalin.

† It has long ago been advised that those specimens of simple minerals only should be selected for chemical examination, or analysis, which are free from all admixture either of the rock which forms their matrix, or of any other foreign ingredient. The same caution should be carefully extended to rock specimens which have their characters in many instances as much or more disguised and altered by the intimate admixture of imbedded minerals.

‡ In some places, the hornblende is distributed in patches, giving the rock a cloudy appearance; its particles being still too minute to be identified as hornblende even with a lens.

§ This limestone is rapidly acted upon by dilute muriatic acid. Thus treated, it yields a considerable residuum, consisting of carbon and an earthy matter, readily fusible by the blowpipe into a semi-transparent globule, of a white colour, which may be considered, perhaps, as felspar in a state of minute division.

The limestone is succeeded by dark-grey and black argillaceous slate, occasionally passing into a hornblende slate, and containing many subordinate beds, or masses of small grained porphyritic greenstone. In the latter, the imbedded substance is invariably felspar; the same mineral seems predominant also in the base.‡ Some specimens procured in Mr. Saville's copse, nearly opposite to Pen Clee Flats, are by no means destitute of beauty. At this spot also my companion § discovered a portion of the black slate containing minute crystals of chiastolite, a mineral which had not before, as far as I am aware, been noticed in the west of England. I have since found it in a similar rock occupying nearly the same relative situation to the granite near Ivy Bridge, on the south of Dartmoor. Passing on towards the new road, cut by Mr. Saville, on the left bank of the river, we found the slate losing its intense black colour, and assuming all the characters of transition clay slate. At the section afforded by the abovementioned road, it is found of various shades of grey and fawn colour, occasionally containing spangles of mica. Here it alternates with beds of compact greywacke,‖ and exhibits the curved and undulating stratification so strikingly characteristic of this series throughout its whole extent in the west.

To this imperfect sketch, I have only to add, that the rock which I have described as greenstone exhibits no traces of hornblende either as an imbedded substance, or in the veins or rifts. As insulated specimens, many or most of its varieties might, with more accuracy, be considered as compact or slaty felspar. Considered, however, in its geological relations with the rocks of Sourton, Sticklepath, and other portions, apparently of the same formation, surrounding this extremity of Dartmoor, most of which abound in hornblende, it may fairly, under the present state of our knowledge and nomenclature, be termed greenstone. Whether it may not become expedient hereafter to assign a separate class to those rocks of which compact felspar forms the principal ingredient, or whether a careful pursuit of the experiments of Cordier and others may not pave the way to a more accurate subdivision of the obscurer members of the greenstone and trap families, are questions which I willingly leave to your more acute and better informed readers. To such as have the opportunity, I would further recommend the careful examination of the rocks immediately incumbent on the granite of Dartmoor, especially those which occur between that rock and the limestone so frequently occurring in its neighbourhood.

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* I have found traces of the same mineral and also of axinite in a slaty compact felspar, more or less tinged with hornblende (?) near Ivy Bridge. The specimens of compact felspar from this spot (I. B.) are among the best characterized and handsomest which I observed in the west of England.

† A rock apparently identical with this occurs on the southern boundary of Dartmoor between the greenstone of Bovey Tracy and the granite.

‡ The rock may in general be referred to granite, Div. I. D. c. of Dr. Macculloch's Classif. In some blocks we observed this greenstone apparently traversed by veins of a yet darker and more compact variety much resembling common basalt.

§ The Rev. P. Serle.

‖ I have used the term greywacke, because, from the general aspect of this portion of rock, most geologists would at once, I apprehend, consider it as belonging to that formation. Both the slaty and compact varieties are, however, fusible, though, with difficulty, by the blowpipe. The latter is, perhaps, an intimate mixture of granular felspar and quartz.

The following should have been added to the memoranda on the Red Marle of Devonshire:

The only rock hitherto ascertained to be subordinate to the red marle of Devonshire is the amygdaloid, which has been observed in various parts of its extent, but most conspicuously in the neighbourhood of Thorverton and Silverton. This rock was first noticed more than 20 years back, and not inaccurately described (if my memory serve me aright) by Dr. Maton, in his tour through the western countries.

Its general aspect is that of a granular mass, somewhat loosely compacted, of a purplish-brown colour, more or less intense (given most probably by the oxide of manganese in which it abounds). In this paste are imbedded, or rather intermixed, in such quantities as to form a very considerable part of the whole mass, minute portions of calcareous spar, mica, or chlorite, in a state of semi-disintegration, and indurated clay (lithomarge ?), sometimes tinged by copper, and sometimes by manganese. This latter substance, as well as the calc spar, frequently traverses the rock in small veins. The cells of the amygdaloidal portions are filled or lined with brown oxide of manganese, with calc spar and a coarse jasper. The nodules of the latter are not remarkable either for their size or beauty. The character of the rock is so obscured by this abundant admixture of substances apparently adventitious as to render it very difficult to pronounce with any certainty as to its essential constituents. These we should, I apprehend, in the present state of our knowledge, assume to be granular or earthy felspar, and one or more of the following: hornblende, augite, bronzite, or hyperstene, probably the second of these. My specimens do not afford distinct indications of any of them. The more compact portions fuse before the blowpipe, sometimes into black glass more or less slaggy, sometimes into a dirty-white enamel more or less mixed with black patches. The criterion, therefore, proposed by Cordier, here fails us.* A portion of the rock broken into small fragments, and exposed for an hour to the heat of a Black's furnace, gave a black glass much resembling that produced by various forms of the dolerite under the same circumstances.

The same obscurity which is attached to the mineralogical character of this rock seems to extend in some measure to its relations with the conglomerate in which it occurs. In some places it covers, and in others is covered by sandstone. On the road from Killerton to Silverton, near a house occupied (in the year 1812) by Mrs. Brown, we saw it resting on the large grained conglomerate; and at one of the Radden quarries, near Thorverton, covered by a sandstone bed of from three to ten feet in thickness. Its line of separation from the sandstone is sometimes tolerably distinct. In one quarry at Thorverton, a line of sandy clay, not quite a foot thick, prevents their actual contact. At other places, especially at the Radden Quarries, the two substances appear to pass so insensibly into each other as to induce for the moment a conjecture that both were the result of a common deposition modified in its characters by the partial intrusion of some extraneous matter. This phenomenon has already been noticed by Mr. Greenough. "What mineralogist," he asks, "can draw a line of demarcation between the red marl and the toadstone at Heavitree." (Essay, p. 215). Your geological readers have probably already anticipated that a vulcanist would at once decide that the whole of the amygdaloidal beds was a series of *whyn-dykes*; while others will be disposed to regard them as concretions or depositions more nearly connected and contemporaneous with the strata which envelope them. The difficulty would probably vanish before a more accurate investigation of their character and position, which I beg to recommend to such mineralogists as may travel westward.

It may be added that at the Radden Quarries we noticed the occasional tendency of this rock to split into basaltiform balls; and in one spot observed it traversed by nearly horizontal veins of its own substance differing slightly from the mass by their greater compactness, and the largeness of the nodules which they contained. The veins of extraneous matter were mostly vertical, or at a very high angle.

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* I am acquainted with the experiments of M. Cordier only through the notice given of them in M. Bruce's Geologie de l'Ecosse.

ARTICLE II.

On the Aëriform Compounds of Charcoal and Hydrogen; with an Account of some additional Experiments on the Gases from Oil and from Coal. By William Henry, MD. FRS.*

THE experiments on the aëriform compounds of charcoal and hydrogen, described in the following pages, are supplementary to a Memoir on the same class of bodies, which the Royal Society did me the honour to insert in their Transactions for 1808, as well as to other papers on the same subject, which have been published in Mr. Nicholson's Journal, and in the Memoirs of the Manchester Society. Of these essays, I beg leave to offer a very brief recapitulation, with the view merely of connecting them with what is to follow.

In the first of these essays (Nicholson's Journal, 8vo. June, 1805), I detailed a series of experiments on the gases obtained by the destructive distillation of wood, peat, pit-coal, oil, wax, &c. from which it appeared that the fitness of those gases for artificial illumination was greater, as they required for combustion a greater proportional volume of oxygen; and that the gases generated from different inflammable bodies, or from the same inflammable substance under different circumstances, are not so many distinct species, which, under such a view of the subject, would be almost infinite in number, but are mixtures of a few well known gases, chiefly of carburetted hydrogen with variable proportions of olefiant, simple hydrogen, sulphuretted hydrogen, carbonic acid, carbonic oxide, and azotic gases; and that the elastic fluids obtained from coal, oil, &c. have probably, in addition to these, an inflammable vapour diffused through them when recent, which is not removed by passing them through water.† In the same paper, I explained certain anomalies that appear in the experiments of the late Mr. Cruickshank, of Woolwich, which are not at all chargeable as errors upon that excellent chemist, and could only be elucidated by further investigation of the gases to which they relate. Of his labours it would be unjust, indeed, to speak in any terms but those of approbation, for they may fairly be considered as the foundation of most that is now known respecting this species of aëriform bodies. To Mr. Dalton, also, we are indebted for an accurate acquaintance with carburetted hydrogen gas, and for much information that is valuable in assisting us to judge of the composition of mixed combustible gases, by the phenomena and results of firing them with oxygen.‡

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* From the Philosophical Transactions, 1821.

† Nicholson's Journal, 8vo. xi., 72.

‡ New System of Chemical Philosophy, *passim.*

In the second Memoir (Philosophical Transactions, 1808), I described a series of experiments on the gases obtained from several different varieties of pit-coal, and from the same kind of coal under different circumstances. Various species of that mineral were found to yield aëriform products, differing greatly in specific gravity, combustibility, and illuminating power; the cannel coal of Wigan, in Lancashire, being best adapted to the purpose, and the stone-coal of South Wales the least so. In decomposing any one species of coal, the gaseous fluids were ascertained not to be of uniform quality throughout the process, but to vary greatly at different stages; the heavier and more combustible gases coming over first, and the lighter and less combustible afterwards. By subsequent experiments on the gases obtained from coal on the large scale of manufacture, it was found that a similar decline in the value of the products takes place, but not to the same extent, owing, probably, to the greater uniformity of temperature, which is attainable in large operations.*

On the practical conclusions, which it was the object of the last mentioned Essay to establish, I forbear to dwell, because they are unconnected with my present purpose, which is limited to the chemical constitution of these compound gases, and to the methods of separating them accurately from each other. The view of their nature and composition, which was taken in the first Essay, was opposed by those able philosophers, M. Berthollet, and Dr. Murray, of Edinburgh, who both contended for greater latitude as to the proportions in which hydrogen and charcoal are capable of uniting, and considered these proportions indeed as subject to no limitation. The facts, however, which have since been multiplied in this, as well as in other departments of chemistry, tending to prove, that bodies capable of energetic combination unite in a few definite proportions only, leave little doubt that the same law holds good with respect to the compounds of hydrogen and charcoal. Not that it is meant that the known compounds of those elements are the only possible ones; for others will probably be discovered which will still be found conformable to the general law, *that when one body combines with another in different proportions, the greater proportions are multiples of the less by an entire number.*

A different view of the subject has lately been taken by the ingenious author of the Bakerian Lecture, published in the Philosophical Transactions for 1820. In that paper, Mr. Brande has endeavoured to prove that the gas called light carburetted hydrogen, or simply carburetted hydrogen, or hydrocarburet, is not entitled to be considered as a distinct species; that the only aëriform compound of charcoal and hydrogen, which is with certainty known to exist, is the gas called olefiant, or bicarburetted hydrogen; and that the gases evolved by heat from coal and oil are in fact nothing more than mixtures of olefiant and simple hydrogen gases in various proportions.

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* Manchester Society's Memoirs, New Series, vol. iii.

In assuming, in the first Essay, the existence of light carburetted hydrogen as a definite compound, characterized by its requiring, for the complete combustion of each volume, two volumes of oxygen, and giving one volume of carbonic acid, I relied on the sole authority of Mr. Dalton; for the gas of marshes, though before known to be inflammable, had not been subjected to accurate examination by any other chemist. Mr. Cruickshank, indeed, speaks of it as "pure hydrocarbonate;"* but since he classes it in that respect with the gas obtained by the destructive distillation of camphor, from which it differs essentially in composition, it is plain that he was not correctly acquainted with the properties of pure carburetted hydrogen. Previously to the second set of experiments, I satisfied myself by the careful analysis of a specimen of the gas from stagnant water, for which I was indebted to Mr. Dalton, that it really has the properties which have been ascribed to it by him as characteristic; and in 1807 I found precisely the same characters in the fire-damp of coal-mines.† Dr. Thomson, also, from experiments in 1811,‡ on the gas from stagnant water, and Sir Humphry Davy,§ from the analysis of the fire-damp in 1815, drew the same conclusions. It is in the power, indeed, of every chemist to investigate for himself the properties and composition of carburetted hydrogen gas, since it may easily be procured in considerable quantity, by stirring the bottom of almost any stagnant pool, especially if composed of clay. During the last summer, I obtained it from a source of this kind, which afforded it in such abundance, that several gallons might have been collected in a few minutes. This gas I submitted to repeated and most careful examination. It contained one-twentieth its volume of carbonic acid, but no sulphuretted hydrogen whatever, and no proportion of oxygen gas that could be discovered by attentively testing it with nitrous gas. The results of its combustion with oxygen gas, effected in a Volta's eudiometer in the usual manner, showed that it was contaminated with 1-15th its volume of azotic gas. Apart, however, from this, the pure portion, in a great number of trials, required, as nearly as can be expected in experiments of this sort, two volumes of oxygen for combustion, and gave one volume of carbonic acid. Its specific gravity, taken on quantities procured at three several times, varied only from ·582 to ·586, the mean of which is ·584; and this, allowing for 1-15th of azotic gas of specific gravity ·972, gives ·556 for the specific gravity of pure carburetted hydrogen gas, a number which coincides almost exactly with that found by Dr.

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* Nicholson's Journal, 4to. vol. v. p. 6.

† Ibid. 8vo. xix. 149.

‡ Memoirs of the Wernerian Society, i. 506.

§ Phil. Trans. 1816, p. 5.

Thomson.* Since, therefore, the same results have been obtained from the examination of gases similarly collected at distant times and places, there appears to me no reason for refusing to consider carburetted hydrogen gas as a true chemical compound, characterized by perfect uniformity of properties and composition. At the temperature of 60° Fahrenheit, and under 30 inches pressure, 100 cubical inches must weigh 16·95 grains, and be composed (taking the weight of 100 cubic inches of carbonic acid at 46·5 grs. and the charcoal in 100 grs. of that acid at 27·3 grs.) of

	Grains.	Grains.	Grains.
Charcoal ..	12·69	74·87	100·00
Hydrogen..	4·26	25·13	33·41
	16·95	100·00	133·41

And olefiant gas (giving twice its volume of carbonic acid by combustion, and weighing 29·64 grs. for 100 cubical inches†) must be constituted of

	Grains.	Grains.	Grains.
Charcoal ..	25·38	85·63	100·00
Hydrogen..	4·26	14·37	16·71
	29·64	100·00	116·71

And as 16·7 is to 100, so very nearly is 1 to 6, which last number is the weight of the atom of charcoal, as deduced from the constitution of olefiant gas. It is true, that this determination a little exceeds that which is derived from the composition of carbonic acid (viz. 5·65), the atom of oxygen being taken at 7·5. But if 8 be the true number for oxygen, which now seems to be most probable both from experiment and analogy, we shall then find an exact coincidence between the relative weight of the atom of charcoal, as deduced from olefiant gas, and as determined from carbonic acid. Perhaps the true specific gravity of hydrogen gas, on which depend the relative weights of the atoms of hydrogen and oxygen, may be fully as correctly ascertained from the composition of carburetted hydrogen, as by direct attempts to weigh so light a fluid. Now, as the hydrogen in 100 cubic inches of hydrocarburet weighs only 4·26 grs. and is equivalent to 200 cubic inches of hydrogen gas, we have 2·13 grs. for the weight of 100 cubic inches of hydrogen gas, from which may be deduced ·0698 for its specific gravity, that of air being 1. And if the specific gravity of oxygen gas be 1·111, it will be found that the two volumes of hydrogen required to saturate one volume of oxygen gas, have as nearly as possible the relative weight of 1 to 8.

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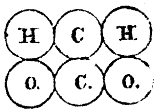
* *Annals of Philosophy*, xvi. 252.

† I adopt this result of Dr. Thomson from its near coincidence with that of an experiment of my own, on the specific gravity of olefiant gas, published in the Phil. Trans. 1808, p. 293.

Were any additional argument necessary to establish the existence of carburetted hydrogen as a distinct species, it might be derived from the action of water on that gas, which, besides being absorbable in a constant proportion, admits of being expelled again by the application of heat, not otherwise changed than in having acquired a small quantity of those gases which are always present in water, and of which it is impossible to deprive it even by long continued boiling.

The process, by which carburetted hydrogen gas is evolved in natural operations, is no doubt the decomposition of water, and admits of being explained on the atomic theory of Mr. Dalton, by supposing two atoms of charcoal to act at once on two atoms of water. One atom of charcoal attracts the two atoms of hydrogen, forming carburetted hydrogen gas, and the other atom of charcoal unites with two atoms of oxygen, constituting carbonic acid. This is illustrated by the annexed figure, in which two atoms of charcoal, C, C, are represented as interposed between two atoms of water, each consisting of an atom of hydrogen and an atom of oxygen. Dividing the diagram vertically into three parts, we have the original substances; and separating it horizontally, we obtain the two new compounds. This theoretical view of the subject is confirmed by the fact, that the carburetted hydrogen, formed at the bottom of stagnant pools, is never accompanied by carbonic oxide, but always by carbonic acid, the full quantity of which is prevented from appearing, in consequence of the absorption of a great part of it by the mass of water, under which the changes are taking place.



Being provided with such an abundant supply of carburetted hydrogen, I availed myself of it to examine the mutual action of that gas and chlorine on each other, principally with a view to ascertain how far reliance may be placed on the latter as an instrument in the analysis of mixed combustible gases. This is a part of the subject that was first investigated, though with a different view, by Mr. Cruickshank.* He observed that a mixture of chlorine with hydrogen, carburetted hydrogen, or carbonic oxide in certain proportions, kept in a bottle entirely filled with the mixture, and furnished with an air-tight stopper, did not exhibit any immediate action, but that in 24 hours, on withdrawing the stopper, the fluid immediately rushed in, and filled most of the space originally occupied by the gases. But he was not aware of the influence of light on these changes, which was discovered about the same time by Gay-Lussac† and by Dalton.* It does not, however, appear to have been ascertained by either of them, whether the complete exclusion of light prevents any degree of action of chlorine and carburetted hydrogen on each other. I mixed, therefore, those two gases in different proportions in well stopped vials, which were completely filled with the mixture, and covered by opaque cases. When the stoppers were removed under water, at various intervals after the mixture, from a few minutes to 39 days, no diminution whatever of volume was found to have taken place; and after having removed the chlorine by liquid potash, the carburetted hydrogen gas gave the usual products of carbonic acid, and consumed the usual proportion of oxygen. Mixtures also of hydrogen and chlorine, and of carburetted hydrogen and chlorine, standing over water in graduated tubes, which were shaded by opaque covers, sustained no loss of bulk, except what arose from the absorption of chlorine by the water, the combustible gas remaining wholly unaltered. It may be considered, therefore, as quite essential to the mutual agency of these gases, that they should be subjected to the influence of light. But it is not necessary that the direct rays of the sun should fall on the mixture, the light of a dull and cloudy day being fully adequate to the effect. On a day of this sort, I filled several stoppered vials, graduated into hundredths of a cubic inch, with a mixture of 30 volumes of carburetted hydrogen with from 80 to 90 of chlorine, and uncovering them all at the same moment, exposed them to the feeble light which was then abroad. By exposure of one of the vials during half a minute, no diminution of volume was found to have been effected; another vial, opened under water when one minute had elapsed, showed an absorption of five parts; a third in two minutes had lost 15 parts; a fourth in four minutes 25 parts; and a fifth, opened in five minutes, contained only 50 volumes out of the original 110.

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* Nicholson's Journal, 4to. v. 202.

† Mem. de la Soc. d'Arcueil, ii. 349.

The products, resulting from the contact of carburetted hydrogen and chlorine, under circumstances favourable to their mutual action, have been described by Mr. Cruickshank, with whose experience on this point my own entirely agrees. When rather more than four volumes of chlorine are kept in mixture with one volume of gas from stagnant water, the products are muriatic acid gas, and a volume of carbonic acid equivalent to that of the pure carburetted hydrogen; and this, whether the mixture be exposed to direct or indirect solar light; the only difference being that the less intense the light, the more slowly is the effect produced. When less than four volumes of chlorine are employed, the residue consists of muriatic and carbonic acids, carbonic oxide, and undecomposed carburetted hydrogen, the proportions of the two last increasing as, within certain limits, we reduce the relative quantity of chlorine. These changes were ascertained, both by Dr. Davy and the late Dr. Murray,* to depend on the presence of moisture, which is unavoidably introduced in the common mode of operating; for when the gases, first perfectly dried, were mixed in an exhausted glass vessel, and exposed even to the direct rays of the sun, no mutual action was found to ensue. In the theory of these changes there is, it must be confessed, a little uncertainty. Does the chlorine, it may be asked, act simultaneously on the hydrogen of water, and on that of the combustible gas; or does it decompose water only? The former view of the subject appears to me most probable, because, if the chlorine acted on water only, free hydrogen would be evolved from that portion of the hydrocarburet which abandons its charcoal to the oxygen of the water; which is not consistent with experience. When it is required to form carbonic acid, four volumes of chlorine must be used for the decomposition of each volume of carburetted hydrogen. In this case, two atoms of chlorine unite with the two atoms of hydrogen existing in the combustible gas, and the two other atoms of chlorine with the two atoms of hydrogen from the water. But to convert carburetted hydrogen into carbonic oxide, three atoms of chlorine are sufficient, two of which are employed, as in the first case, and the third is expended in saturating the hydrogen of one atom of water, which supplies to the charcoal an atom of oxygen for the formation of carbonic oxide. Calculating in the same manner, we shall find, also, that three atoms of chlorine are adequate to convert one atom of carbonic oxide into carbonic acid.

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* New System of Chemical Philosophy, p. 300.

The facts which have been stated sufficiently prove, that chlorine cannot be employed as a means of correctly analyzing mixtures of olefiant gas, either with hydrogen or with carburetted hydrogen, if light be admitted, even though of feeble intensity, and for the short interval during which such an experiment may be expected to continue: and they explain that uncertainty as to the results of analyses of mixed gases made in this way, which was first remarked by Mr. Faraday,† and subsequently by myself.‡ Chlorine becomes, however, a most useful agent in separating olefiant gas from such mixtures, provided light be entirely excluded during its operation, as I have found by subjecting to its action mixtures of those gases with known proportions of olefiant gas. In these analytical experiments, I admitted into a graduated tube standing over water, a volume of chlorine exceeding by about one half what was known to be sufficient, and noted its bulk when actually in the tube, which was immediately shaded by an opaque cover. A measured quantity of the mixture was then passed up, and in about 10 minutes the outer cover was cautiously lifted, till the surface of the water appeared. The diminution of volume thus ascertained, divided by 2, was found to give pretty correctly the quantity of olefiant gas known to be contained in the mixture. But the greatest precision was attained by waiting 15 or 20 minutes, and then quickly washing the remaining gas with dilute solution of potash, in order to remove the excess of chlorine. From the volume of the residuary gas, it was necessary to deduct the amount of impurity previously ascertained to exist in the chlorine; and the remainder, taken from the volume of mixed gases which had been operated on, showed how much olefiant gas had been condensed by the chlorine. When very narrow tubes were employed, and the column of gases mixed with chlorine was of considerable length, a longer continuance of the experiment was found necessary, and the gases were suffered to remain in contact during an hour or more. In this way it was ascertained, that olefiant gas may be accurately separated by chlorine from hydrogen, carburetted hydrogen, or carbonic oxide gases, or from mixtures of two or more of those gases, which are left quite unchanged in volume and in chemical properties, when light has been carefully excluded from the mixture.

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* Nicholson's Journal, xxviii. 143, and 201.

† Journal of Science, &c. vi. 358.

‡ Manchester Memoirs, New Series, vol. iii.

This property of chlorine is the foundation of a fresh analysis, to which I have thought it expedient to submit the gases from coal and oil, in order to decide what aëriform fluids remain after the separation of that portion which is condensible by chlorine; —whether the residue consists, as I have heretofore maintained, of carburetted hydrogen chiefly, with variable proportions of hydrogen and carbonic oxide; or whether, according to the new view of the subject, it consists of hydrogen gas only.

In the experiments made for this purpose, I operated generally on from 60 to 80 cubic inches of oil gas or coal gas, assaying a small specimen first, as a guide to the quantity of chlorine which it would be necessary to employ. The volume of chlorine thus found to be requisite, and about half as much more, was passed into an air receiver standing over water, and completely shaded by an opaque cover which was fitted over it. The oil or coal gas was then added by degrees, if much condensation was expected, because in that case a considerable increase of temperature would have been produced by the sudden admixture of large quantities; or at once, if only a moderate action had been indicated by the previous assay. The mixture was allowed to stand, completely guarded from the light, during 30 or 40 minutes, or even longer, and the residue was expeditiously washed with liquid potash, and a small portion again assayed, to ascertain that the action of the chlorine was complete. The specific gravity of the washed gas was then carefully taken, that of the entire gas having been previously determined: and the results of its combustion with oxygen examined, and compared with those of the gas in its original state.

Experiments on the Gas from Oil.

In obtaining this gas at different times, I used the same kind of whale oil, which had been heated a little below its boiling point during two hours, in order to deprive it of water. The oil was admitted by drops into an ignited iron tube filled with fragments of broken crucibles, and no difference, that I am aware of, existed in the circumstances under which the decomposition was effected, except that the degree of heat was purposely lowered in the latter processes, till that temperature was attained, which was barely adequate to the production of gas. The oil gas procured from London, I obtained through the kindness of Mr. Richard Phillips. It had been prepared from cod oil, at the manufactory of Messrs. John and Philip Taylor, and having been conveyed to Manchester in bottles accurately stoppered and tied over with a double fold of bladder, it was found not to have acquired any admixture with atmospheric air. The results are contained in the following table, in which the expression *entire gas* is applied to the gas precisely as it came over, except that the carbonic acid had been removed by liquid potash, applied in the smallest quantity and with the least agitation that were adequate to the effect.

TABLE I.—*Containing the Results of Experiments on the Gas obtained from Whale Oil.*

Entire gas.					Residue left by chlorine.		
			100 vols.			100 vols.	
No. of Experiment.	Sp. Grav.	100 vols. lose by chlorine.	take oxyg.	give carb. ac.	Spec. Grav.	take oxyg.	give carb. ac.
1	·464	6	116	61	·4107	94	46
2	·590	19	178	100	·4400	108	58
3	·758	22·5	220	130	·6160	145	85
4 (London)	·906	38	260	158	·6060	152	91

From the foregoing table it appears, that the gas obtained at different times from oil of the same quality is far from being of uniform composition, and that great differences, as to its specific gravity and chemical properties, are occasioned by the temperature at which it is produced. So far as my experience goes, no temperature short of ignition is sufficient for the decomposition of oil into permanent combustible gases; but the lower the heat that is employed, provided it be adequate to the effect, the heavier and more combustible is the gas, and the better suited to artificial illumination.

From the experiments which I published in 1805, and which were made on a single specimen of oil gas, I was led to consider it as constituted of one volume of olefiant gas with seven volumes of mixed gases, of which the greatest part was carburetted hydrogen. Mr. Dalton has since favoured me with a specimen of oil gas prepared by himself, which contained in 100 parts, 40 of a gas condensible by chlorine; and it appears from the table that oil gas, manufactured on the large scale, may contain in 100 parts, 38 parts of a gas similarly characterized.* It is not improbable indeed that by a temperature carefully regulated, the whole of the aëriform fluids may be obtained from oil, of such quality as to be entirely condensible by chlorine; and from the great superiority of the light which such a gas would afford, and the reduction that might be effected in the capacity of the gasometers, the discovery of a mode of producing it in this state, would be an important practical improvement.

The inferences respecting the nature of the gas from oil, I reserve till after the account of the experiments on coal gas, as the same remarks, with some slight modifications, will apply to both cases.

Experiments on the Gas from Coal.

The numerous experiments and observations on the gas from coal, which I have already published, appear to me to preclude the necessity of going much into the subject on this occasion. What I have lately had in view, has been to render the analysis of this gas more complete, by a careful examination of that portion of it which remains after the action of chlorine. The gas, submitted to these recent experiments, was prepared from Wigan cannel, at the manufactory of Messrs. Philips and Lee. It was collected from an opening in a pipe between the retort and the tar-pit, generally about an hour after the commencement of the distillation, except in the instance of the gas No. 4, which was taken five hours, and No. 5, which was taken 10 hours from that period. Before using it, the carbonic acid and sulphuretted hydrogen, which were always present in the early products, were separated by careful ablution with liquid potash. As the gas No. 5, was not at all diminished by chlorine, it was obviously unnecessary to examine it in any but its entire state.

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* Since this paper was written, I have received from Mr. Phillips a second specimen of oil gas prepared by Messrs. Taylor. It contains in every 100 volumes, 42 or 43 parts of gas condensible by chlorine; but in other respects very nearly agrees (making allowance for the greater proportion of that ingredient) with the gas described in the text.

Table II.—*Containing the Results of Experiments on the Gas obtained from Coal.*

Entire Gas.					Gas left by Chlorine.		
		100 vols.				100 vols.	
Experiment.	Sp. Grav.	Take oxyg.	Give car. acid.	Loss by chlorine.	Sp. Grav.	Take oxyg.	Give car. acid.
1	·650	217	128	13	·575	178	92
2	·620	194	106	12	·527	160	82
3	·630	196	108	12	·535	148	80½
4	·500	166	93	7	·450	140	75
5	·345	78	30	0			

Inferences respecting the Composition of that Part of the Gases from Coal and from Oil, which is not condensible by the Action of Chlorine.

The analytical experiments, which I have described on the action of chlorine on artificial mixtures of olefiant with hydrogen and carburetted hydrogen gases, afford no room for doubt that by that agent the quantity of olefiant gas in any mixture of these gases may be accurately determined. We are not, however, acquainted with any chemical agent, either liquid or aëriform, which, from a mixture of hydrogen, carburetted hydrogen, and carbonic oxide, is capable of separating one of those gases, leaving the others in their original state and quantity.* The only method at present known of determining the composition of such a mixture is by firing it with oxygen gas, and, from the phenomena and results of the process, deducing the proportion of its ingredients. In drawing conclusions of this kind, it is necessary to have distinctly in view the properties of those gases in their separate state. The following Table contains an abstract of their leading characters, which will be found very useful in such investigations. Though not strictly necessary, I have included olefiant gas, in order to render the Table more complete.

Table III.—*Exhibiting the characteristic Properties of different combustible Gases.*

Names of gases.	Sp. Gr. Air 1000.	100 vols. require oxygen.	Total.	Diminished by firing.	Carb. acid produced.
Olefiant gas.	·970	300	400	200 = ½	200
Carburetted hydrogen	·556	200	300	200 = ⅔	100
Hydrogen gas.	·069	50	150	150 = ¾	0
Carbonic oxide	·972	50	150	50 = ⅓	100

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* I have not found that chlorine can be employed with any success in analyzing such mixtures; for when placed in contact with two or more of those gases, and exposed to light, it does not act upon one exclusively, but upon all that compose the mixture.

As an illustration of the method of investigating the proportions of mixtures of the three last gases, we may take the instance of a mixed gas, free from olefiant gas, of specific gravity ·534, of which 100 volumes consume 110 of oxygen, and afford 70 of carbonic acid, the diminution of the whole 210 after firing being 140 volumes. Now it must be obvious from inspection of the Table, that the 70 parts of carbonic acid cannot all have resulted from the combustion of carburetted hydrogen, since, for the saturation of 70 measures of that gas, 140 of oxygen would have been required, whereas only 110 have been expended. We may, therefore, safely infer the presence of carbonic oxide, a gas which, by combustion, gives its own volume of carbonic acid, with the expenditure of only half its volume of oxygen. The specific gravity of the specimen being lower than that of carburetted hydrogen, indicates also an admixture of simple hydrogen gas; and of this the proportion must necessarily be considerable, to countervail the weight of the heavy carbonic oxide. The following proportions of the three gases will be found to coincide with the properties of the mixture.

	Consume ox.	Give carb. ac.	Dim. by firing.
40 vols. of carb. hydrogen	80	40	80
30 vols. of carb. oxide	15	30	15
30 vols. of hydrogen gas	15	0	45
100	110	70	140

No reliance, however, can be placed on the accuracy of such estimates, unless the specific gravity of the specimen agrees with that of the hypothetical mixture, as deduced from the proportion of its ingredients. But when this coincidence takes place, we have all the evidence, which the subject at present admits, of the nature of the mixture; and as this agreement between experiment and calculation was found to take place very nearly in all the instances comprehended in the two following Tables, we may consider the numbers composing them, as expressing, with sufficient exactness, the relative proportion of different gases in the residues of oil and coal gas left by the action of chlorine.

TABLE IV.—*Showing the Composition of 100 Volumes of the Gas remaining after the Action of Chlorine on Oil Gas.*

Exper.	Azote.	Carb. hydr.	Carb. oxide.	Hydr. gas.	Total.
1	7	30	15	48	100
2	5	40	15	40	100
3	5	65	20	10	100
4	5	75	15	5	100

TABLE V.—*Showing the Composition of 100 Volumes of the Gas remaining after the Action of Chlorine on Coal Gas.*

Exper.	Azote.	Carb. hydr.	Carb. oxide.	Hydr. gas.	Total.
1	1·5	94·5	4	0	100
2	6	82	2	10	100
3	2	66	14	18	100
4	5	60	12	23	100
5	10	20	10	60	100

It appears from the two foregoing Tables, that the portion of oil gas and coal gas, which is not condensible by chlorine, is in every case a mixed gas, consisting in most instances of carburetted hydrogen, carbonic oxide, and hydrogen, with a little azote, part of which may be traced to the impurity of the chlorine. In the best specimens of oil gas, the carbonic oxide is in greater proportion than in the best kinds of gas from coal, and the carburetted hydrogen is most abundant in the latter gas. This, however, is more than compensated, so far as their illuminating power is concerned, by the greater richness of the aëriform products of oil in that denser species of gas, which is separable by chlorine. The proportion of hydrogen, both in oil gas and coal gas, appears to increase as they are formed at a higher temperature, and is always greatest in the latter portions of the gas from coal. But no instance has ever occurred to me of a gas obtained from oil or from coal, which, after the action of chlorine upon it with the exclusion of light, presented a residuum at all approaching to simple hydrogen gas; nor do I believe that such a gas can be generated under any circumstances of temperature, by which the decomposition of coal or of oil is capable of being effected.

Inferences respecting the Composition of that Part of the Gas from Coal and Oil, which is condensed by Contact with Chlorine.

When a given volume of a mixture of olefiant and carburetted hydrogen gases is fired with oxygen, and an equal volume of the same mixture is first deprived of olefiant gas by the action of chlorine, and then fired with oxygen, it must necessarily happen that the excess of oxygen spent in the first combustion, above that consumed in the second, will be three times the volume of the olefiant gas, and that the excess of carbonic acid formed in the first experiment above that generated in the second, will be double the volume of the olefiant gas. A remarkable anomaly, however, was, during the last summer, observed by Mr. Dalton in the results of the combustion of a quantity of gas, which he had himself prepared from oil. One volume was found to consume three volumes of oxygen, and to yield little short of two volumes of carbonic acid, in those respects agreeing nearly with olefiant gas; but when mingled with more than the requisite proportion of chlorine, it was not, as olefiant gas would have been, entirely condensed, but suffered a diminution of only four-tenths of its bulk, the remaining six-tenths, after being freed from the redundant chlorine, agreeing in its properties with carburetted hydrogen. For example, 10 volumes of this gas (containing four of gas condensible by chlorine and 6 of carburetted hydrogen) consumed 30 volumes of oxygen, and gave 18 of carbonic acid. But of the oxygen, 12 volumes are due to the 6 of carburetted hydrogen, leaving 18 volumes for the combustion of the four volumes of gas condensible by chlorine, which is in the proportion of 4½ to 1. Of the 18 volumes of carbonic acid, also, 6 may be traced to the combustion of the carburetted hydrogen, leaving 12 volumes as the product of four of the condensible gas, or in the proportion of 3 to 1. The portion of gas, condensed by the action of chlorine, presents, therefore, decided differences from olefiant gas, in requiring not three only, but 4½ volumes of oxygen for combustion, and in affording 3, instead of 2 volumes of carbonic acid. Nearly the same relation of the oxygen consumed, and carbonic acid produced, to that part of the gases from coal and oil which is condensible by chlorine, existed also not only in other experiments of Mr. Dalton, but in all those which I have myself made. The proportions I have found to vary in different cases from 4½ to 5 volumes of oxygen, and from 2½ to 3 volumes of carbonic acid for each volume of the condensible gas.

On comparing also the specific gravity of the gases from coal and oil, as ascertained by experiment, with that which ought to result from mixtures of the residue left by chlorine, with such a proportion of olefiant gas as is deducible from analysis, I have invariably found that the real specific gravity has considerably exceeded the estimated. For instance, the London oil gas was composed of 38 volumes of a gas condensible by chlorine, and 62 volumes of mixed gases not characterized by that property, and having the specific gravity ·606. But 62 volumes of gas, of specific gravity ·606, mixed with 38 volumes of olefiant gas, of specific gravity ·970, should give a mixture of the specific gravity ·754, instead of ·906, which was the actual specific gravity of the entire oil gas. It will be found on calculation that the 38 volumes of gas, in order to make up the real specific gravity of the oil gas, must have had the specific gravity of 1·4 very nearly. This is the highest number that is deducible from my experiments for the specific gravity of that portion of oil gas or coal gas, which is condensed by the action of chlorine. In other instances, it varied from that number down to 1·2, but in every case its weight surpassed that of common air.

It is evident from these facts that the aeriform ingredient of oil gas and coal gas, which is reducible to a liquid form by chlorine, is not identical with the olefiant gas obtained by the action of sulphuric acid on alcohol, but considerably exceeds that gas in specific gravity and combustibility. Two views may be taken of its nature; for it may either be a gas *sui generis*, hitherto unknown, and constituted of hydrogen and charcoal in different proportions from those composing any known compound of those elements;—or it may be merely the vapour of a highly volatile oil, mingled in various proportions with olefiant gas, carburetted hydrogen, and the other combustible gases. Of these two opinions, Mr. Dalton is inclined to the first, considering it as supported by the fact that oil gas, or coal gas, may be passed through water, without being deprived of the ingredient in question; and that this anomalous elastic fluid is absorbed by agitation with water, and again expelled by heat or other gases, unchanged as to its chemical properties, as we have both satisfied ourselves by repeated experiments. On the other hand, I have found that hydrogen gas, by remaining several days in narrow tubes in contact with fluid naphtha, acquires the property of being affected by chlorine precisely as if it were mixed with a small proportion of olefiant gas; and I am informed by Dr. Hope, that oil gas, when forcibly compressed in Gordon's portable gas lamp, deposits a portion of a highly volatile essential oil. The smell also of the liquid which is condensed on the inner surface of a glass receiver, in which oil gas or coal gas has been mixed with chlorine, denotes the presence of chloric ether, evidently however mingled with the odour of some other fluid, which seems to me to bear most resemblance to that of spirit of turpentine. This part of the subject is well worthy of further investigation; but having devoted to the inquiry all the leisure which I am now able to command, I must remain satisfied at present with such conclusions as are safely deducible from the foregoing investigation. These may be briefly recapitulated as follows:

1. That carburetted hydrogen gas must still be considered as a distinct species, requiring for the perfect combustion of each volume two volumes of oxygen, and affording one volume of carbonic acid; and that if olefiant gas be considered as constituted of one atom of charcoal united with one atom of hydrogen, carburetted hydrogen must consist of one atom of charcoal in combination with two atoms of hydrogen.

2. That there is a marked distinction between the action of chlorine on olefiant gas (which, in certain proportions, is entirely independent of the presence of light, and is attended with the speedy condensation of the two gases into chloric ether), and its relation to hydrogen, carburetted hydrogen, and carbonic oxide gases, on all which it is inefficient, provided light be perfectly excluded from the mixture.

3. That since chlorine, under these circumstances, condenses olefiant gas without acting on the other three gases, it may be

employed in the correct separation of the former from one or more of the three latter.

4. That the gases evolved by heat from coal and from oil, though extremely uncertain as to the proportions of their ingredients, consist essentially of carburetted hydrogen, with variable proportions of hydrogen and carbonic oxide: and that they owe, moreover, much of their illuminating power to an elastic fluid, which resembles olefiant gas in the property of being speedily condensed by chlorine.

5. That the portion of oil gas and coal gas, which chlorine thus converts into a liquid form, does not precisely agree with olefiant gas in its other properties; but requires, for the combustion of each volume, nearly two volumes of oxygen more than are sufficient for saturating one volume of olefiant gas, and affords one additional volume of carbonic acid. It is probably, therefore, either a mixture of olefiant gas with a heavier and more combustible gas or vapour, or a new gas *sui generis*, consisting of hydrogen and charcoal, in proportions that remain to be determined.

Manchester, Jan. 1821.

Article III.

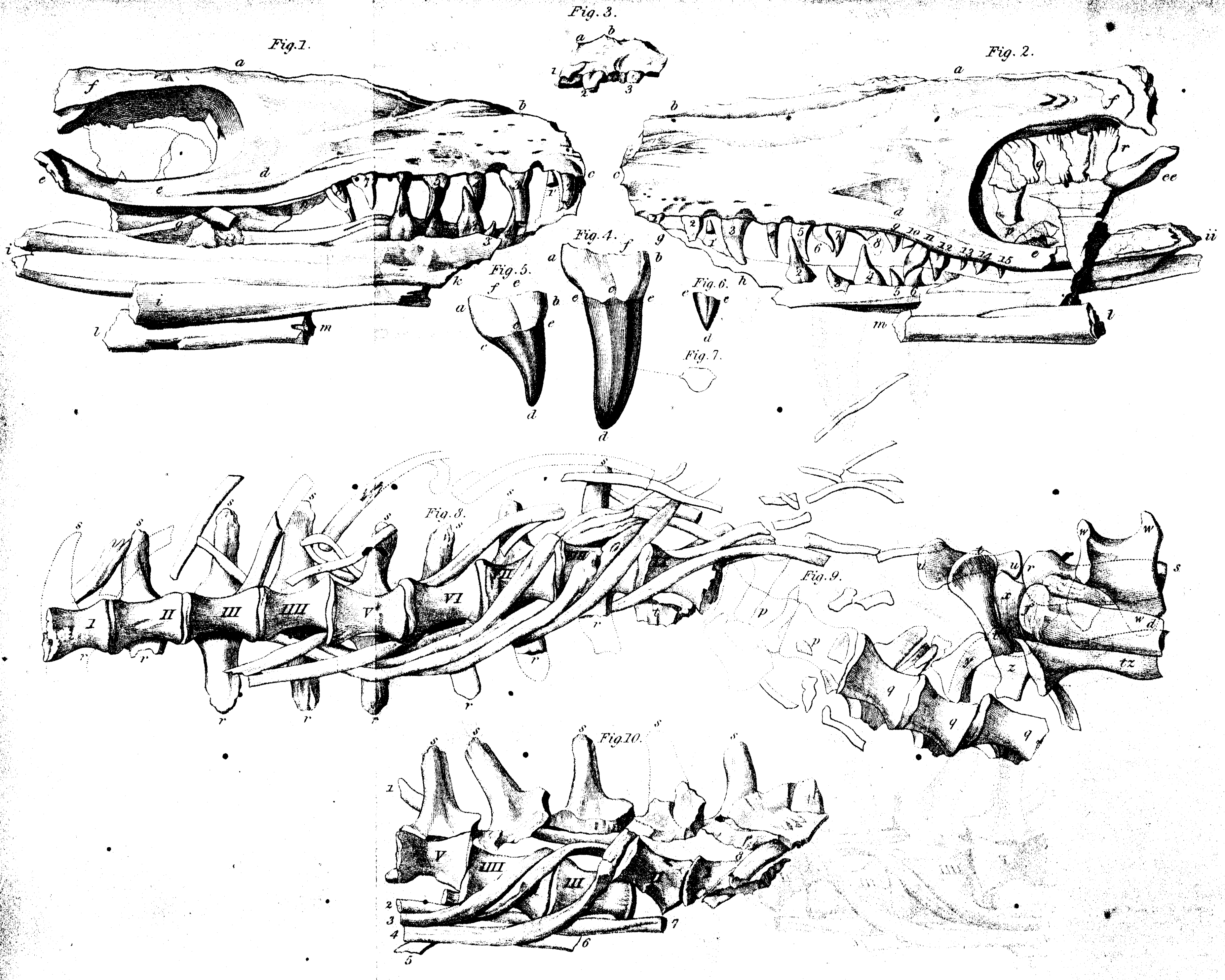
On the Production of Colours by Mechanical Division. By Mr. J. P. Charlton.

(To the Editor of the *Annals of Philosophy.*)

SIR,

In the course of some experiments upon enamel colours, I have been led to observe a fact which you may, perhaps, think worth insertion in your *Annals*, as it is contrary to the statements of approved chemical works, and as it must, I think, be considered a remarkable instance of a complete change of colour produced merely by mechanical comminution: the fact to which I allude is, that oxygenation is not essential to the rose colour which gold imparts to enamels.

It has long been known that silver has the property of staining glass an opaque blue or green, when viewed by reflected light, which becomes a fine transparent orange colour, when viewed by transmitted light, a property generally attributed to the oxide, but which I have found to belong equally to metallic silver, which, when fired alone in contact with glass, is quite as effectual as all the other preparations of it. From the above and some other circumstances, I was led to suspect that the case might be the same with respect to gold. Accordingly, I ground toge-



Engraved for the Annals of Philosophy. Published by Baldwin, Cradock & Joy, Sept. 1st 1821.

ther one part of metallic gold with 20 parts of common enamellers' flux, and obtained a rose-coloured enamel without the slightest metallic appearance. The gold was easily ground as it was in that friable state, to which it is reduced by some process with which I am unacquainted, and now commonly sold by the refiners. As the above experiment shows that metallic gold is capable of imparting a rose colour, it is natural to conclude that in all other cases, the colour is in reality owing, not to the oxide, as usually stated, but to metallic gold in a state of minute subdivision.

The above result inclined me to form the same inference, with respect to the enamel colours which may be obtained from platina, and to suspect that the beautiful black * described by Mr. Cooper in the Journal of the Royal Institution, No. V. is, in fact, owing to minutely divided platina in the metallic state. I, therefore, mixed three parts of flux with one part of the deep black powder, described in the same paper, as a hydrate of platina, in the hopes of producing the same rich black colour, but I obtained only an enamel of a dark grey colour much like plumbago. This result was certainly contrary to my expectation, and would lead to the conclusion that oxygenation of the platina is necessary to produce a fine black, but I do not think the experiment decisive, and hope that further trials will enable me to speak with more certainty. I am, Sir,

Your most obedient servant,

J. P. Charlton.

Article IV.

On the Lacerta Gigantea of the Ancient World.

By S. T. Von Sömmering.† (With a Plate.)

[This paper was recommended to the Editor's notice, and lent to him for translation, by his friend Mr. Parkinson, author of the "Organic Remains."]

Bavaria, a country which possesses such a number of the finest remains of a former world, is now able to show also those of that wonderful monster, of which hitherto no traces have been discovered, except in the environs of Maestricht and Vicenza, the same animal concerning which, in 1812, Cuvier ‡ said, "La determination precise du fameux animal de Maestricht nous paroit surtout aussi remarquable pour la théorie des loix zoologiques que pour l'histoire du globe."

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* The same black enamel may be obtained by boiling insoluble muriate with caustic potash.

† Read June 25, 1816, at the Royal Academy of Sciences.

‡ Recherches sur les Ossemens Fossiles de Quadrupedes, tom. iv. Paris, 1812. Pref. p. 5. See also my treatise on the Crocodilus Priscus, or the Gavial of Antiquity, in the Memoirs of the Royal Academy of Sciences, Sect. 21 and 22.

Hoping that this novel fact, with which we have become acquainted by means of the contents of the present stone blocks, the most incontrovertible documents from the archives of a former world, will prove not unworthy the attention of the Royal Academy of Sciences, I have the honour of submitting to its notice the specimens themselves, and likewise drawings of them on the same scale as the originals. And I have been the more encouraged to give this accompaniment to my Treatise on the Crocodilus Priscus, by the flattering approbation which my colleagues bestowed on that essay. For the specimens themselves, I am indebted to the politeness and liberality of Count J. Ad. Reisach.

As far as I have been able to ascertain, they were discovered in one of the Bohn Ore (Bohnerz) mines of the Meulenhard, near Deiting, in the district of Manheim, the same in which the Crocodilus Priscus was found. Their bed was about 10 feet below the surface; consequently more than double the depth of that of the latter animal.

It is exceedingly to be regretted that the blocks of stone containing these, and probably separated from each other many centuries ago, were not only broken into several pieces of different bulk, but that many of them were completely destroyed; for, when by a portion of the stone accidentally breaking off and exposing some of the teeth, the discovery was first made, it was too late to recover from out of the mass the five or six other pieces belonging to it, and already thrown away.

Actual inspection convinces us, however, that all these bones must have belonged to the skeleton of the same individual, since the greater part of the stones fitted to each other, and the bones adhering to them corresponded both as to configuration and proportion.

As the examination of fossil bones had always been one of my favourite pursuits, I did not rest until I had caused the stone to be removed from these fragments, as far as it was possible to do so without injuring the bones themselves, and thus reduced them to that intelligible state in which they now appear.

Owing to the softness of the marly mass in which they were incorporated, not so hard as chalk, this was effected with incomparably less labour, and risk of injury, than attended the discovery of the crocodile found in the same district, but not at the same distance from the surface of the earth. For whereas the substance, in which the latter was buried, required to be removed by the chissel and hammer, that encrusting these fragments was so tender that it could be scraped away with a knife. It was only the portions of the pea iron ore (eisenbohnerz) adhering in some places very strongly to the bones, that required great pains and extreme caution to separate them; and after all it was necessary to leave them remaining in some parts where it would have been impossible to detach them without inevitable injury to the bone. Wherever such a lump of iron ore had adhered, there afterwards remained a dark, rusty-coloured mark. I found no appearance of there having been any similar lumps in the block containing the skeleton of the crocodile.

This soft chalky marl, in consequence of a mixture of lighter or darker iron ochre, of a yellowish white hue, is besides foliated in the manner of slate, and exceedingly easy to break. Here and there may be discerned, in addition to the lumps of iron ore, pieces of greyish quartz. Everywhere too might be discerned fine scales of not more than one line in their greater diameter, that must have belonged either to fish, or, perhaps, to the animal itself. Similar scales are to be found in great abundance in the chalky marly slate of the Meulenhard—a circumstance, which I have ascertained by examination on the very spot. Besides these, there is in one place the entire impression of a flat, radiated ammonite, between three and four inches large, and the greyish blue remains of a shell.

The bones themselves, which are, correctly speaking, rather calcined than petrified, have a general resemblance in colour and consistence to those of the crocodilus priscus; and are plainly enough distinguishable as well by their darker hue (either a reddish-grey, or reddish-brown), and by their closer, firmer, and harder texture (notwithstanding their friability), from the marly mass, which is of a lighter colour, and more soft. Not only the bones of the head, but still more those of the pelvis and the thighs, are dyed of a dark brown by the iron ore, which had adhered to them. The enamel of the teeth, which is brown, smooth, and shining, appears to be more compact than any other part, and in these respects bears a striking resemblance to that on the fossil teeth of the shark, or of the glossopetre.

Head.

Notwithstanding that the fragment of the head appears to have been forcibly compressed so as to be flattened, and to have its parts thrust from their natural position, it is nevertheless not only evidently in better preservation, but more entire than any portion which I have yet seen of the head of the Maestricht animal. In none of those fragments, at least none that have as yet been engraved, do we see both the right and left sides of the face or facial parts of the skull; in none are there any remains of the upper jaw; in none do the upper and under jaw correspond so completely with each other; in none is there discernible so large a portion of the snout and of the forehead above the eyes.

This more perfect preservation of the parts of the face in this subject enables us to ascertain with greater precision the class of animals to which this head bears the most resemblance. For on considering the heads of the different species of the lizard tribe, it is principally the fore part of the head that characterizes the varieties occurring among them, as in the hinder part the general resemblance is less diversified.

It is owing to this striking difference in the face that we can distinguish at first sight the head of the gavial from that of the crocodile, and these again from those of the gecko, the iguan, the stellio, and the draco; not only in a fresh and unprepared state, but still more plainly in the skeleton. Thus it is principally in the parts forming the face that we perceive the most decided difference between the scull of the lacerta monitor, tupinambis, draco, and stellio. Similar differences in these parts are to be discerned in Camper's admirable plates of the sculls of the tupinambis, tequixin, and iguan.[*]

Since, therefore, it is the *facies* that principally shows the resemblance, or the difference between the various species of lizards; and as fortunately the greater part of this is here preserved so as to be plainly distinguishable; for whatever imperfection there happens to be on one side is supplied by the existence of the corresponding parts on the other side, it becomes less difficult for us to compare these lacerta of the former world with those of the present one.

Among all the varieties with which I am acquainted, I find that, except with regard to size, the *facies* of our lacerta bears the greatest resemblance to that of the lacerta monitor. This resemblance is most obvious on the left side (Pl. VIII[†]), fig. 2, where, on comparing them, we find an evident analogy in the general conical form of the upper jaw; the hollow of the eye equally large in proportion; an equally large cheekbone; the same furrow, or slight hollow, between the bones of the nose, and a similarly shaped under jaw.

Judging from the scull of a lacerta monitor now before me, and likewise from the plate of an iguan by Fischer,[‡] or that of a large teguixin by Camper,[§] I am of opinion that not only the entire intermaxillary bone is wanting, but also a portion of the right and left upper jaw. This part, which is also deficient in most of the plates of the Maestricht animal, as well as the incisores teeth belonging to it, can be restored from the representation given by Camper,[‖] who possesses a natural specimen.

Teeth.

If the teeth in our animal be compared with those of the lacerta monitor, a striking difference will be evident; for although the manner in which they are fixed into the jaw, and also that of their arrangement appear to be similar, their form is obviously unlike. No tooth, for instance, has a crown terminating in several points, as is the case with almost every one of the lacerta monitor, but each adheres to the jaws by a swelling, flatly rounded root, has a pyramidal top somewhat inclined forwards, and is coated with a brownish enamel. The *root* or *kernel* of these teeth differ as well by their less dark, bright grey colour, as by their greater thickness, not only from the substance of all the other bones; but even from that of the jaw.

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[*] Annales du Museum, à Paris, tom. xix. Pl. 11, fig. 5, 6, 8, fig. B, fig. C.

[†] This Plate has been reduced.—*Ed.*

[‡] On the different Forms of the intermaxillary Bones of Animals, Leipsig, 1800, tab. iii. fig. 8.

[§] Annales du Museum d'Hist. Nat. à Paris, tom. xix. 1812, Pl. 11. fig. 8.

[‖] Ibid. fig. 3.

The top, which is covered with a brown, dark, porcelain species of enamel, has almost the appearance of a dagger. The exterior surface of this part of the tooth is divided from the inner one by a dark, sharp, jagged edge (almost the same as in the glossopeter); and is not only less convex than the inner surface, but has moreover obtuse-angled facettes in its longitudinal direction. In order to exhibit this more distinctly, I have given a magnified representation of the best preserved of these teeth both the external face, fig. 4; and a section through it, fig. 7.

In both the sculls of tupinambis, with which I have been favoured by the kindness of Prof. Schneider, of Breslaw, I find similarly formed teeth, only in these the dark jagged angles are not easily discernible without a magnifying glass. This resemblance between the teeth of our unknown animal and those of the tupinambis warrants us in assigning it to the genus lacertæ.

As far as I can form an opinion from the teeth of the Maestricht animal;[*] from those which I formerly possessed myself, afterwards given to M. Ebell, of Bremen; and from others of them which I saw in the possession of my distinguished teacher, Petrus Camper;[†]—from his masterly representations of the same, —from those in the works of his son Adrian Camper,[‡] Faujas St. Fond,[§] or Cuvier,[||] the teeth of our incognitum bear the most complete resemblance to those of the animals discovered at Maestricht and Vicenza.

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[*] In speaking of the Maestricht animal, Cuvier terms this solid part of the tooth not the root but the kernel (noyau).—(Ann. du Mus. tom. xii. p. 156.)

[†] This eminent naturalist gave a most admirable representation of the teeth of the Maestricht animal, of the natural size, in the Phil. Trans. 1786, vol. 76, Pl. 15 and 16, p. 446, which has been well copied in his smaller pieces in Heracle's translation, and also copied, but less accurately, in "Les Œuvres de M. Camper, Paris, 1803, fol. Pl. 6 and 7. A better plate of the same subject, though not even this is sufficiently accurate, has been given by Faujas St. Fond, Hist. de la Montaigne de St. Pierre, &c. Paris, 1799, Pl. 6.

[‡] Journal de Physique, An. ix. 1800, tom. 51, p. 278, Pl. 1 and 2. The size is less than half the natural dimensions. The hindmost tooth, Pl. 2, fig. 6, is the best drawn of all.

[§] Histoire Nat. de la Montagne de St. Pierre à Maestricht, Paris, 1799.; also in his Essai de Geologie, Paris, 1805, Pl. 7. In Pl. 4 and 5, of his Hist. de la Mont. where more than a dozen teeth are exhibited, hardly any one of them is accurately represented. Even in that marked *c c*, in Pl. 49, the root is separated too suddenly from the upper part, and, in most instances, the teeth have the appearance of projecting up from the root as if out of a distinct case. The best representation he has given is Pl 50, fig 1, yet nothing is to be observed of the jagged edge, which is so much the more extraordinary as in the very same plate he has exhibited it very beautifully in the teeth of the shark (squalus), Pl. 18, fig. 1 and 9, and as Camper's figure must have pointed it out to him.

[||] Ann. du Mus. d'Hist. Nat tom. xii. Pl. 19, where the teeth of the Maestricht animal appear as if the root was contracted just at the commencement of the upper part.

Of such teeth, there are seven on the right side, in the upper jaw belonging to our animal; and at least fourteen on the left side. In the under jaw, there are only five on the right and six on the left; these too are not all in equally good preservation, some of them being broken.

There being the evident remains of three teeth in the little fragment marked fig. 3, which, in all probability, belonged to the left side of the upper jaw, we may reasonably conjecture that the animal had certainly more than 17 teeth in this jaw.

The teeth appear smaller in front than in the intermediate parts from whence again they gradually decrease as they proceed towards the extremities, so that the most backward teeth are less than any of the others.

Besides the two fragments belonging to the head; namely, the larger one in fig. 1 and 2, and the lesser represented in fig. 3, there is another of a middling size, which apparently belongs to the palate, but could not be engraved on account of its indistinctness and imperfect state.

The right side portion of the under jaw of the Maestricht animal, as given by Faujas St. Fond (Pl. L), which is apparently very perfect, exhibits 14 teeth nearly regularly increasing in size as they recede from the front, so that the foremost teeth are the smallest, and the hindermost the largest. Judging not only from this, but even from the 14 teeth in the left side of the upper jaw belonging to our fragment, more than half of the under jaw is wanting in our fragment.

The palate teeth which are to be seen in the Maestricht animal, and which are so important towards characterizing the animal, appear either to be wanting, as do likewise the bones of the palate, or to be still buried up among the rubbish that could not easily be removed.

Vertebræ.

Nineteen of the vertebræ are very evident, fig. 8 and 10, and of two others, fig. 8, there are perceptible traces or impressions.

Eight of these vertebræ, fig. 8, part of which belong to the back, and part to the loins, appear, notwithstanding their distortion, to be arranged as they were in the living animal. Their *lateral processes*, which are of considerable size, are very distinguishable.

The ninth of these, fig. 9, as likewise the two of which only the traces are perceptible (*p p*), appear to belong to the loins and pelvis, as do the other three, marked *q q q*, to the tail, both on account of their situation (behind the bones of the pelvis, and below those of the thigh), and of their flat form.

Five other vertebræ found in a separate mass of stone seem to have belonged to the foremost part of the backbone. Yet as this fragment does not exactly correspond with the rest of the bones, I dare not pronounce this to be decidedly the case.

As far as can be ascertained, these vertebræ are concave both before and behind, and not, as Cuvier observes those of the sauria and ophidia to be, viz. concave in front, and convex behind. In dimensions, all these vertebræ appear to be proportioned to the head, and the complete resemblance which they bear to each other in form, size, junction, processes, substance, &c. seem to prove that the five discovered in a separate stone belong to the same individual as the other fourteen.

Ribs.

More than 30 of the ribs are discernible. The larger ones are for the most part displaced, and lying along the spine, and at the same time partly bruised and broken off. The binder and lesser ones lie scattered at some distance from the spine.

Pelvis.

Among the bones of the pelvis, which are likewise removed from their situation, the two share bones, as likewise the os ischii, on the right side, are perfectly entire; but of the right hip bone there is only a part remaining.

Thigh Bones.

Of the right and also of the left thigh bone, there is only the upper half. Whether some fragments found close to those belonged to the cross bone, is now exceedingly difficult to determine on account of their imperfect state.

Besides these remains of bones, there are to be perceived not only in the single stones, but every where throughout the entire mass, small, delicate, and, for the most part, roundish scales, yet not resembling those that have formed themselves so abundantly around the fossil gavial in my possession.

On comparing the present considerable fragment of the head of our animal, fig. 1 and 2, with those belonging to the celebrated one discovered at Maestricht, and now deposited in the Museum at Paris, the most remarkable of any, and on that very account already engraved ten* different times, we not only discover the most striking resemblance (independent of size) between these two specimens, as well in the general configuration as in the details; but that the one now exhibited, which is fortunately less imperfect, is of greater assistance in determining the real form of the head of this extraordinary animal than the so justly celebrated fragment deposited in the Museum at Paris, although that is invaluable on account of its magnitude. Among other parts wanting in that specimen, both the forehead and snout, as likewise the circle surrounding the cavity of the eye, may be admirably supplied from those parts as exhibited in our animal. Our animal likewise contains 17 teeth in the upper jaw, whereas that has only nine.

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* The first engraving is a coarsely executed print in Les Dons de la Nature par Buchoz.

The second, hardly at all superior to the preceding, is in the Magazin Encyclopedique, tom. vi. p. 34.

The third, which, as well as all the others, with the exception of the seventh, I have before me, is in Faujas St. Fond. Hist. de la Montagne de St. Pierre, Pl. 4.

The fourth, in the same work, Pl. 4. Cuvier says of this, that it is "tres belle, mais mal terminée dans le haut."

The fifth is only the preceding on a reduced scale in Faujas St. Fond's Essais de Geologie, tom. 1, Pl. 8, on an octavo leaf, where the teeth are represented disproportionably thick.

The sixth is the elegant little vignette serving as a head-piece in St. Fond's Hist. de la Mont. St. Pierre.

In fact, the general form of our animal bears a strong resemblance to that of the other, as may be seen by referring to Plate LI, of St. Fond. And this perfect resemblance in the contour of both specimens furnishes at the same time an incontrovertible proof that our animal was very young, and had hardly attained a quarter of its size, as this admirable engraving of St. Fond's is three-fourths less than the original, and is yet covered as nearly as may be, by it, or, at least, by the exact representation which we have given of it.

'The rest of our fossil fragments are likewise particularly important, as they exhibit such portions of the bones of the pelvis and thigh as were, if I remember right, hitherto quite unknown. Even M. Cuvier, who possesses by far the greater portion of the bones of the Maestricht animal, said in 1808 that no part of the legs had been preserved.

From what has been said both here and in the 21st and 22d paragraphs of my treatise on the Crocodilus Pricus, the following results appear to arise: the large gigantic lacerta, which has been discovered on St. Peter's Hill, at Maestricht, and at Rozzo, in the territory of Vicenza, was, during the first ages of the world, an inhabitant of a district belonging to the kingdom of Bavaria, where it was a neighbour of the Crocodilus Priscus. Consequently this race of animals were to be found throughout a tract of country extending from Holland, through Bavaria, to the Vicentine; so that our individual was here in the very centre of the district assigned to it; and, therefore, was not so restricted as to space as has been hitherto supposed.*

For aught that I know to the contrary, these fragments are

The seventh exhibits only the palate. Van Marum, Mem. de la Soc. Teylérienne, an. 1790, Pl. 2.

The eighth represents the hinder part of the lower jaw after a drawing by Camper, the father, in Adrian Camper's Journal de Physique, an. ix. 1800, tom. li.

The ninth is by Cuvier, in the Annales du Mus. d'Hist. Nat. tom. xii. Pl. 19, and is, as I have circumstantially stated in my treatise on the Crocodilus Priscus, sect. 21, the most instructive of any.

The tenth is to be found in Parkinson's Organic Remains of a former World, Pl. 19, fig. 1.

* On n'en a decouvert, jusqu' ici (1808), les ossemens que dans un seul canton assez peu etendu.

the first of the kind that have been found in this territory. It would, therefore, be desirable that whoever, either now is, or may hereafter be, in possession of similar, would contribute what they can to our information respecting this *genus deperditum* by kindly communicating their observations to the Royal Academy of Sciences, accompanying the same with either drawings or casts.

If we compare the dimensions of the bones belonging to the Maestricht and to the Vicentine animal with our own fragments, we shall have reason to suppose that ours was as yet very young, and had hardly attained a quarter of its full growth, which Cuvier computes to be 23 feet in length.

Moreover as these fragments contain two thigh bones, of which no part is preserved in the remains found on St. Peter's Hill, they satisfactorily prove that this animal was no cetaceum, no fish, but actually a lacerta.

The form of the head has so great a resemblance to that of the lacerta monitor, and differs so entirely from the heads of cetacea, fish, and even the crocodile, that there no longer remains any room to dispute the propriety with which Adrian Camper has assigned the Maestricht animal to the lacerta genus—a judgement to which Cuvier completely assents.

As there is not the least appearance of scales belonging either to the neck or the back, of which there are an abundance to be perceived in the small crocodile or gavial in my possession, it is impossible to conjecture this animal to have belonged to the crocodile species. Ammonites were found in the vicinity of this lacerta animal, as is universally the case in all the known instances of fossil crocodiles, gavials, and lacertæ, one proof of which, among others, is the Dresden petrifaction, of which a copy has been made for this Academy.

The compressed and distorted form of the head, and the violence that is apparent in many places, are remarkable, since they indicate some great external force to which either the animal itself, or its skeleton, must have been subjected, as is likewise the case in the crocodilus priscus. And what power must have been exerted not only to flatten the conical head, but even to force out and to break the teeth, as we see has been done here!

If I may be permitted to decide, from my own anatomical and pathological knowledge, I should say that this compression of the head was not effected during the dry, friable, and brittle skeleton state, since, in such a case, owing to the equal force, the upper jaw would have been broken in another direction; or, at least, would not have been so perfect as it now is on the left side. The injury rather appears to have been done in the living animal, on account of the periosteum and top of the head still retaining together the fragments of the bones, notwithstanding their crushed condition.

Hence it appears to me to be a subject well worth attentive examination to discover how it happens that in all the fossil animals of antiquity, except some later ones discovered in a lighter soil, the heads in particular are not only crushed, but at the same time dislocated in their parts. How dreadfully shattered, for instance, are the fragments of the head and jaw discovered at Maestricht, the jaw of the Vicentine animal, the head of the specimen belonging to M. Spener, the heads of the palæotherium and anoplotherium found at Mont Martre, and the head of the crocodilus priscus; while the spine and bones of the limbs have received less injury, and are also less deranged from their natural position.

It is impossible that this should have been occasioned by the solution of a calcareous strata (kalkauflösung) carrying the animal, or its skeleton, along with it. Or even supposing that this might have been the case, such a current could not have carried away the shattered parts, and afterwards deposited them so unitedly, as we find them lodged in horizontal strata of (chalk) calcareous slate.

Since even Cuvier * himself considers the nondescript animal, whose remains we have been here examining, to be not only the most celebrated of any, and to have occasioned the greatest difference of opinion, but to be at the same time the most gigantic of any, "le plus gigantesque de tous," I have the less hesitation in assigning to it the specific name of lacerta gigantea of a former world.

Lastly, when it is considered that, according to Cuvier's calculation, which is certainly not an exaggerated one, this gigantic lacerta was 23 feet in length, we are forcibly reminded of the dragons so much spoken of in fable. At least, the fact that, at one period of the world, there existed animals of the lacerta or dragon kind, more than 20 feet in length, is more astonishing than all that is recorded in ancient tradition respecting monsters which even the wildest fancy did not amplify to such enormous dimensions.

EXPLANATION OF THE PLATE. (VIII.)

Figure 1.

A fragment of the skull as seen on the right side.

a, b. The uneven edge of the superficies of the fragment of the forehead and nose, to which belonged the part compressed on the left side, fig. 2, *a, b.*

a, b, c, d. The upper jaw broken off in front at *b, c,* in such a manner that more than the intermaxillary bone appears to have been destroyed.

e, e. The cheek-bone.

e, e, f. The cavity of the eye, forcibly compressed above so as to appear smaller and lower than that on the left side, or than it was in its perfect state. The portion *f*, appears to have been particularly affected by violence.

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* Ann. du Mus. tom. xii. p. 145.

The hinder part of the line of teeth seems to be concealed by some fragments of the palate being unnaturally forced down, so that the teeth, which are plainly to be seen on the left side (fig. 9 to 15), are not visible here.

i, k. Remains of the right half of the under jaw.

l, m. A portion of the remains of the left half of the under jaw corresponding with *l, m*, in fig. 2.

2, 3, 4, 5, 6, 7, 8. Seven tolerably well preserved teeth of the right upper jaw, which appear to correspond with those similarly figured on the left side.

6 and 8, perhaps also 4, appear to be the points of succeeding teeth, covered with enamel. To be compared with Camper, *a, a*, O. Tab. 2, A, C, D, E. Tooth 8 exhibits the preceding one still beneath or behind it. Of this right side, the teeth marked 3, and 7, are represented on a magnified scale in fig. 4 and 5.

1′, 2′, 3′, 4′, 5′, 6′. Six teeth of the right under jaw; of that marked 1′, there is only the point remaining.

Figure 2.

The left side of the same fragment, as is exhibited in the preceding figure, on the right side.

a, b. A portion of the scull broken from the right side and fallen down on the left. Of this, the edge, *a, b*, corresponds with that similarly marked in the first figure.

*** A gentle depression, or furrow, which is also found in the heads of lizards, and that here extends up the forehead along the bone of the nose.

* * * *c, d*. The left upper jaw broken off in front at *b, c*, so that more than the whole of the intermaxillary bone is wanted.

e. The left cheekbone.

e, f. The front edge of the cavity of the eye, which appears to have retained its original form tolerably well.

g, h, m, l. Remains of the left half of the forcibly shattered under jaw.

p, q, r. Fragments, probably belonging to the cranium.

e, e. The cheek-bone of the opposite, or right side.

i, i. The under jaw of ditto.

1, 2, to 15. Teeth of the left upper jaw, still plainly discernible. 2, 3, 4, 5, 6, 7, and 8, appear to correspond with those similarly figured on the other side. 4, 5, 12, 13, 14, and 15, only are remaining in their original situation, and without injury.

1′, 2′, 3′, 4′, 5′, 6′. Six teeth of the under jaw on the left side partly displaced, and fractured.

Figure 3.

The smallest fragment of the skull with three teeth. The rounded smooth edge, *a, b,* (probably a part of the orifice of the nostrils), and also the direction of the curve of three teeth, 1, 2, and 3, seem to indicate that this fragment belonged to the left side of the upper jaw.

Figures 4, 5, 6, *and* 7.

Teeth of the upper jaw, shown as magnified beyond their natural size.

Fig. 4 answers to tooth 3. Fig. 5 to tooth 7 of the right side. Fig. 6, on the contrary, to 13 of the left side, the most perfect of any. Fig. 7 is a section of fig. 4 made at the dotted line, so that the upper angular half of the outline denotes the outer; and the lower half, the inner convex surface of the tooth.

a, b. The swelling root of the tooth, not coated with an enamel, by means of which it is fixed into the edge of the jaw.

c, c, d. Points or crown of the tooth, smooth, coated with a brown enamel, bent, obtusely pointed, and acutely angled.

e, e, d. The black jagged edge of the enamel having the appearance of a saw; attention has been paid both to the number of these minute points and to the proportions of this saw-like edge.

f, c, d. A slight furrow extending longitudinally.

Figures 8 *and* 9.

Fragments of the spinal vertebræ, ribs, bones of the pelvis, and thigh-bones, adhering to two pieces of stone.

I, II, III, IV, V, VI, VII, VIII, IX. Vertebræ, from their longitudinal slenderness, and the shape of their processes belonging to the ribs.

s, s, s. Left processes of these vertebræ.

r, r, r. Right processes of ditto.

t. Perhaps a part of one of the bones of the pelvis?

Neither the larger or foremost ribs, nor the lesser ones, which lie separate from each other, require any particular mark.

p, p. Merely marks or impressions of two vertebræ, now lost.

q, q, q. Three distinct and perfect vertebræ, hardly half a line in thickness, and doubtless belonging to the tail.

f, d. The outer side of the upper half of the right thigh bone.

r, s. The inner side of the upper half of the left ditto, for the most part covered by the os ischii, *w, w, w.* The trochanter of the same.

u, u. The outer surface of the right share bone (os pubis?) which is in complete preservation.

t, t. The inner surface of the left ditto, equally perfect.

w, w, w. The outer surface of the right os ischii.

t, t, x. Some part, perhaps, of the os ilium?

y, z. Perhaps one of the processes of the cross-bone (kreuzbein).

Figure 10.

I, II, III, IV, V. Vertebræ probably of the back. I appears to have been the foremost; V the hindermost, since V resembles more in its shape the other vertebræ of the ribs.

s, s, s, s, s. Five lateral processes of the right side.

1, 2, to 8. Fragments of eight ribs.

ARTICLE V.

Historical Sketch of Electro-magnetism.

(To the Editor of the *Annals of Philosophy.*)

SIR,

HAVING been engaged latterly in looking over the various papers that have been written on the subject of electro-magnetism, I found much difficulty in gaining a clear idea of what had been done, and by whom, in consequence of their great variety, the number of theories advanced in them, their confused dates, and other circumstances. This induced me to draw up a catalogue of such of these papers as I could obtain access to, and to make some general arrangement of the matter contained in them. The following attempt does not by any means profess to give a correct view of the subject, or of what has been done in it; nevertheless, perhaps, in the absence of a digested and scientific account, you may think it worth publication. Though it can give no information to those who have worked in the field that has been opened by this new discovery, it may assist in informing others what the labourers have done; and after knowledge has been acquired, it is always desirable that it should be distributed.

M. Oersted, Professor of Natural Philosophy, and Secretary to the Royal Society of Copenhagen, has, for many years, been engaged in inquiries respecting the identity of chemical, electrical, and magnetic forces; and as early as 1807 proposed to try "whether electricity the most latent had any action on the magnet." At that time no experimental proofs of the peculiar opinions he entertained were known; but his constancy in the pursuit of his subject, both by reasoning and experiment, was well rewarded in the winter of 1819 by the discovery of a fact of which not a single person beside himself had the slightest suspicion; but which, when once known, instantly drew the attention of all those who were at all able to appreciate its importance and value.

M. Oersted's own account of this discovery has been published in your *Annals* in vol. xvi. of the First Series. It is full of important matter, and contains, in few words, the results of a great number of observations; and, with his second paper, comprises a very large part of the *facts* that are as yet known relating to this subject. It is necessary, for the sake of connection, in this account, that I state much of what has been described in those papers, though nothing that I shall say will at all supersede the necessity of reading them, to those who wish to gain a knowledge on the subject.

Upon the excitation of the voltaic apparatus by the proper arrangement of its plates and fluid, it is known that certain powers are given to its poles or extremities which enable them, when attached to an electrometer to show by their divergence a certain tension of electricity; or when connected together by fluids, wires, or other conducting substances, to decompose or heat them. These effects have been known for several years, and are generally attributed to electricity produced by the apparatus; the effects of tension belonging to the insulated state of the poles; those of decomposition and heating to their connected state.

When the two poles of such a battery or apparatus are connected by conductors of electricity, the battery is discharged; that is, the tension of the electricity at the poles is lessened, and that according as the conducting power of the substance is more or less. Good conductors, as the metals, discharge it entirely and instantly; bad conductors, with more or less difficulty; but as the instrument has within itself the power of renewing its first state of tension on the removal of the conducting medium, and that in a very short space of time, it is evident that the connecting substance is continually performing the same office during the whole time of its contact that it did at the first moment, and this whether it be a good or a bad conductor; and it is also evident that it must be in a different state in this situation than when separated from the apparatus. It is important at present rather to consider the action of a good conductor in discharging the battery, as the phenomena to be considered are in that case more energetic. A metallic wire, therefore, may be used to connect the two poles; it will discharge a powerful apparatus; and consequently whatever takes place in the connecting medium is here compressed into a very small place. Those who consider electricity as a fluid, or as two fluids, conceive that a current or currents of electricity are passing through the wire during the whole time it forms the connection between the poles of an active apparatus. There are many arguments in favour of the materiality of electricity, and but few against it; but still it is only a supposition; and it will be as well to remember, while pursuing the subject of electro-magnetism, that we have no proof of the materiality of electricity, or of the existence of any current through the wire.

Whatever be the cause which is active within the connecting wire, whether it be the passage of matter through it, or the induction of a particular state of its parts, it produces certain very extraordinary effects. If small, it becomes heated; and as the size of the wire is diminished, or that of the apparatus increased, the heat rises to an intense degree apparently without any limitation, except from the influence of external circumstances, or the alteration of the wire. Another effect, and it is that which has been discovered by M. Oersted, is, that, if brought towards a magnetic needle, it has the power of attracting and repelling it in a constant manner, and in obedience to certain simple laws.

If a magnetic needle be left to take its natural direction, and then a straight portion of the connecting wire be brought above it, and parallel to it, that end of the needle next the negative pole of the battery moves towards the west; and that whether the wire be on the one or the other side of the needle, so that it be above and parallel to it. If the connecting wire be sunk on either side the needle so as to come into the horizontal plane in which the needle is allowed to move, there is no motion of the needle in that plane; but the needle attempts to move in a vertical circle; and, but for the imperfect suspension, and the influence of the earth's magnetism, would do so. When the wire is on the east of the needle, the pole of the needle next the negative end of the battery is elevated; and when on the west of the needle, it is depressed. If the connecting wire be now sunk below the level of the needle, similar attractions and repulsions take place, but in opposite directions to those followed when it is above. The pole of the needle opposite the negative end of the battery now moves eastwards, whatever the position of the wire, so that it be restricted as above.

That these positions of the magnetic needle may be retained with more facility in the memory, Prof. Oersted proposes the following formula: "the pole *above* which the *negative* electricity enters is turned to the *west; under* which, to the *east.*"

Oersted immediately pointed out, what it is easy to see from the above experiments, that the movement of the needle took place in a circle round the connecting wire; and though in the description of his first experiments the quantity of declination given to the needle from the wire is expressed by an angle of so many degrees, yet it is immediately stated to vary with the power of the battery. Whenever the needle is moved in a horizontal or any other circle from the position it naturally assumes, the power of the earth over it tends to restore that position, and is consequently an active force in the present instance opposed to the power of the connecting wire; it, therefore, lessens the declination the needle would otherwise have. Also when the wire is brought into the same horizontal circle with the needle, its effect over it is shown by the elevation and depression of its opposite ends; and it is the mode of suspension combined with the earth's magnetic power that prevents it from traversing in a vertical circle. But if those interfering circumstances be removed, i. e. if the suspension be such as to allow of free motion to the needle in every direction, and the earth's magnetism be rendered null, or counteracted either by the position of the needle, or by the vicinity of another magnet, then a much simpler idea of the relative movements of the wire and needle may be obtained.

It is not, perhaps, easy to obtain this perfect state of the apparatus, but it is not difficult so to arrange it as to examine the movements first in one direction, and then in another. It will then be found that if the connecting wires of a sufficiently powerful apparatus be placed near a magnetic needle so as to pass near its centre, that the needle will arrange itself directly across the wire, whatever the previous position of the two; that if the wire be carried round the centre of the needle, or the centre of the needle round the wire, the same relative position of the two will continue, and that the direction of the needle across the wire is not indifferent, but has its poles always in a constant position to the poles of the battery. If the positive pole of a battery be on our right hand, and the negative pole on the left, and a wire be stretched between, connecting them, then a needle above the wire will point the north pole from, and the south towards; or if below, the south pole from, and the north towards us (Plate IX *); figs. 1, 2. If the connecting wire and the needle be represented by two small rods named accordingly, and fastened permanently together, then they will represent the wire and the needle in all positions; for, however one be placed, the other will correspond with it: or if on the under side of a small square piece of glass a line be drawn from top to bottom, the upper end being called negative, and the lower positive; and on the upper surface a line be drawn from left to right, the left termination being named south, the right north; then the lower line will always represent the connecting wire, and the upper the needle; fig. 3.

The needle and wire being in this position, if the wire be moved along the needle towards either extremity, strong attraction will take place between it and the pole, notwithstanding the same part of the wire be employed; and the poles in the two positions are contrary to each other. In this case it appears that the same point in the wire has the power of attracting both the north and south pole of the needle. If, while the wire is thus situated near the end of the needle, the latter be turned round so that the pole before there be replaced by the opposite pole, strong repulsion will take place, and that to whichever pole the wire has in the first instance been carried; so that the same

* This Plate will be given in the next Number.

point which before attracted both poles will now repel them both. If, when the wire is near the extremity of the needle where the attraction is strongest, it be moved round the end so as to go from one side to the other, keeping the same point constantly towards the needle, its attractive power over the needle will be found to increase as it approaches the end but remains on one side of it, will diminish as it turns the end, will become null when exactly opposed to the pole, and as it passes on the other side will assume repulsive powers which will be strongest at the extremity of the pole on the opposite side to where the wire was situated at first: fig. 4.

In all these cases, the positions assumed by the wire and needle, whether the result of attraction or repulsion, are the same as those before described, except that the wire is now near the end of the needle instead of the middle, as at fig. 5, where there are two positions of the wire, either of which will attract the pole opposite to it, and it will be found that all the attractions and repulsions may be reduced to four positions of the needle to the wire, in which it forms tangents with it: figs. 6, 7, show the positions in which the two poles are attracted; in fig. 6, the north pole; in fig. 7, the south pole; if in either of them the poles of the needle be reversed, the tangents remaining in the same direction, repulsion will take place. Hence it is easy to see how any individual part of the wire may be made attractive or repulsive of either pole of the magnetic needle by mere change of position.

I have been more earnest in my endeavours to explain this simple but important point of position, because I have met with a great number of persons who have found it difficult to comprehend; and it constitutes a very important part of M. Oersted's discovery. Having, however, given the best view of it I am at present able to do, I will hasten to enumerate some other facts of the discovery.

The magnetic property does not depend upon the metal employed or its form, but is exerted by any of them which forms the circuit between the poles: even a tube filled with mercury is effectual: the only difference is in the quantity of effect produced. It continues also, though the conductor be interrupted by water, unless the interruption be of great extent.

The magnetic influence of the wire extends through all sorts of substances, and acts on the needle beyond, just as with common magnetism. It does not act on needles of brass, glass, or gum lac.

In a second paper on this subject, M. Oersted shows that not intensity, but quantity, is wanting in the voltaic apparatus to produce this effect most eminently. A single galvanic arc is sufficient for the purpose. A plate of zinc, six inches square, placed in a trough of copper, filled with diluted acid, enabled the wire which connected the two metals to act powerfully; and with a similar arrangement, the zinc plate having a surface of

100 square inches, an effect was produced on the needle at a distance of three feet. He also, in this paper, describes the construction of a voltaic combination so light, that being suspended, it moved on the approach of a magnet; the motions were in accordance with what has been said, and may easily be conceived.

(*To be continued.*)

Article VI.

On Oxide of Manganese found in the Neighbourhood of Newcastle-upon-Tyne. By Mr. N. J. Winch.

(To the Editor of the *Annals of Philosophy.*)

SIR, *Newcastle-upon-Tyne, May 3, 1821.*

It may not be uninteresting to such of your readers as possess estates or manorial rights in districts, the geological features of which are similar to those of our coal formation, to be made acquainted with the discovery of the oxide of manganese in this neighbourhood. Flying reports had long been in circulation of the existence of this mineral at Ousten, near Urpeth, situated between three and four miles north-west of Chester-le-street, in the county of Durham, but it was generally surmised that iron slag, of which large quantities occur by the sides of all the Roman roads in the north of England, had been mistaken for it, for no traces of this metal had been previously detected in any of our numerous mines or quarries. However, about a week since, these reports were verified by some large masses of the black oxide being uncovered by the plough, but whether connected with a vein, or a bed, is not yet determined. The specimen now before me is black; its fracture conchoidal; and structure cellular; the interstices partly filled with iron ochre. Manganese seems to pervade the newest as well as the oldest rocks; Brogniart mentions it in chalk; the black oxide has been detected in the Orkney Islands, and the gray in the slate mountains of Cumberland. The geological position of this coal formation is above the encrinal, and below the magnesian limestones.

While on the subject of localities of rare minerals, it may not be amiss to mention that diallage forming a subordinate bed in mica schist was met with three or four years ago by Dr. Boue, at Craig Cailleich, in the Highlands; and at Castle Hill, near Keswick, by Mr. Joseph Fryer, who has also noticed veins of beautiful yellow ferruginous quartz in the greywacke at Langholm, on the borders of Scotland.

I have the honour to be, Sir, &c.

Nat. John Winch.

ARTICLE VII.

Tables of Temperature, and a Mathematical Development of the Causes and Laws of the Phænomena which have been adduced in Support of the Hypotheses of "Calorific Capacity, Latent Heat," &c. By John Herapath, Esq.

(*Continued from* p. 103.)

PROP. II. PROB. I.

WHEN portions of the same fluid are mixed at unequal temperatures, we have seen by the preceding prop. that the temperature of the mixture differs from that which would result if the degrees of Fahrenheit indicated the true increments of temperature: let it now, therefore, be required to determine the most advantageous circumstance under which the mixture can be made with given temperatures; or the temperatures being given, let it be required to determine the proportion of the quantities to be mixed, so that the difference between the resulting temperature and the Fahrenheit theory shall be a maximum.

If the Fahrenheit thermometer indicated the true increments of temperature, the temperature of the mixture should be $\frac{FW + F_1W_1}{W + W_1}$; W, W_1, denoting the weights of the portions mixed, and F, F_1, their respective temperatures on Fahrenheit. Therefore, when $W_1 = nW$, the temperature of the mixture becomes $\frac{F + nF_1}{1 + n}$. But under these same circumstances, the true indication on Fahrenheit by Cor. 4 to the preceding Prop. is $(448 + F) \times \left\{\frac{1 + n \times \sqrt{\frac{448 + F_1}{448 + F}}}{n + 1}\right\}^2 - 448 =$ $\left(\frac{\sqrt{448 + F} + n \times \sqrt{448 + F_1}}{n + 1}\right)^2 - 448$. Hence the difference between these results, or $\frac{F + nF_1}{n + 1} - \left(\frac{\sqrt{448 + F} + n \times \sqrt{448 + F_1}}{n + 1}\right)^2$ $+ 448$ is the difference between the two theories; and, consequently, the value of n deduced from the equation $d \cdot \frac{F + nF_1}{n + 1} = d \cdot \left(\frac{p + np_1}{n + 1}\right)^2$ (by putting p for $\sqrt{448 + F}$ and p_1 for $\sqrt{448 + F_1}$) on the supposition of n being the only variable, will show the ratio of W_1 to W, when that difference is the greatest. Therefore, $\frac{F_1 - F}{(n + 1)^2} = 2\frac{p_1 - p}{(n + 1)^2} \cdot \frac{np_1 + p}{n + 1}$ and $F_1 - F$

$= 2\,(p_1 - p)\,.\,\frac{n\,p_1 + p}{n + 1}$; from which we easily get $n\,(F_1 - F) + F_1 - F = 2\,n\,(p_1^2 - p_1\,p) + 2\,(p_1\,p - p^2)$, and $n = \frac{2\,p_1\,p - 2\,p^2 - (F_1 - F)}{2\,p_1\,p - 2\,p_1^2 + F_1 - F}$. Now by putting $p = T$, we have Cor. 2 of the preceding Prop. $p_1 = T_1$, and $F_1 - F = T_1^2 - T^2$; whence $n = \frac{2\,T_1\,T - 2\,T^2 - (T_1^2 - T^2)}{2\,T_1\,T - 2\,T_1^2 + (T_1^2 - T^2)} = \frac{2\,T_1\,T - T^2 - T_1^2}{2\,T_1\,T - T^2 - T_1^2} = 1$. The most advantageous method, therefore, of examining the truth of the theory is when the weights of the portions mixed are equal, as I have stated at p. 406 of the last volume of the *Annals;* for under this circumstance, the difference between the Fahrenheit theory and mine is a maximum.

PROP. III. THEOR. II.

Let N, N_1, denote the numeratoms of any two fluids which do not act chemically on one another, and T, T_1, their true temperatures, then if τ be the true temperature of the mixture of equal volumes of these fluids, $N : N_1 :: \tau - T_1 : T - \tau$.

For no motion being supposed to be gained or lost by the mixture, the sum of the motions of all the particles of the mixture will be equal to the sum of the motions of all the particles in the two fluids before the mixture. But the volumes of the two fluids being equal, the sum of all the motions of each fluid will be as its temperature and numeratom conjointly; and the temperature of the mixture being supposed to be uniform, the sum of all the motions of the mixture will be as the sum of the two numeratoms multiplied by the common temperature; therefore, $\tau\,(N + N\,) = T\,N + T_1\,N_1$, and, consequently, $N : N_1 :: \tau - T_1 : T - \tau$.

Cor.—Hence the numeratoms and temperatures of equal volumes of any two fluids which have no chemical action on one another being given, the temperature of the mixture may be found; for since $N : N_1 :: \tau - T_1 : T - \tau$, we have $\tau = \frac{T\,N + T_1\,N_1}{N + N_1}$. By this cor. as soon as we have determined by one experiment the ratio of the numeratoms, we can employ it to examine the accuracy of the theory by other experiments.

Scholium.

In Dr. Henry's Chemistry, it is said that if equal portions in volume of mercury at 100° F and water at 40° be mixed together, the temperature of the mixture will be 60°. Hence if N denote the numeratom of this water, the preceding tables give $T_1 = 1068{\cdot}5$, $T = 1008{\cdot}3$, and $\tau = 1028{\cdot}8$; and, therefore, $N_1 : N :: 20{\cdot}5 : 39{\cdot}7 :: 1 : 1{\cdot}94$, or as $1 : 2$ nearly. Taking this ratio as correct, we shall be able to compare the theory with the experiment from which it has been deduced, as well as with the other experiments mentioned by Dr. Henry. For, by the preceding cor. $\tau = \frac{T N + T_1 N_1}{N + N_1} = \frac{2 T + T_1}{3}$, by substituting for N the numeratom of the water, and N_1 the numeratom of the mercury; their values 2 and 1.

Hence in the experiment I have quoted $\tau = \frac{2 \times 1008\cdot3 + 1068\cdot5}{3} = 1028\cdot4$, which by Table III. corresponds with 59·6° of Fahrenheit; that is, ·4° below the experiment.

Dr. Henry also tells us, that if the same temperatures be used, but reversed with respect to the bodies; that is, if the water be put at 100°, and the mercury at 40°, the temperature of the mixture will be nearly 80°. With these data, and the preceding tables, we find T = 1068·5 and T_1 = 1008·3, and, therefore, $\tau = \frac{2 \times 1068\cdot5 + 1008\cdot3}{3} = 1048\cdot3$, which brings out the Fahrenheit temperature 79·5°, or ·5° below the experiment.

The same author tells us, that if two volumes of mercury be mixed with one of water at the same temperatures, 100° and 40°, it matters not whichever of these has the higher temperature; the temperature of the mixture will be 70°. Now in this case, as the mercury is double in volume to what the water is, there will be twice the number of particles of mercury in this experiment to what there was in the last; and, therefore, for N_1 we must put twice N_1, and the formula becomes $\tau = \frac{2 T + 2 T_1}{4} = \frac{1}{2}(T + T_1)$. which is a symmetrical function of T and T_1 of the simplest kind. It is no matter, therefore, if two given temperatures be used, whether T or T_1 be put for the higher, the result will be the same, which so far agrees with what Dr. Henry says. Let T = (100° Fahrenheit) 1068·5, then T_1 = 1008·3 and T = $\frac{1}{2} \times 2076\cdot8 = 1038\cdot4$ or 69·6° of Fahrenheit; that is, ·4° beneath the experiment.

Sir H. Davy, in his Elements of Chemical Philosophy, has mentioned an experiment that would require a different ratio for the numeratoms from that which I have given. Dr. Murray, in his Chemistry, has given results still different from those of Sir H. Davy. MM. Lavoisier and Laplace, Dalton, Irvine, Wilcke, Kirwan, and Crawford, have all given results so widely differing, that the extreme specific capacities determined from these experiments have a ratio no less than that of 5 to 2; so that on only 50° of Fahrenheit, the extreme experiments would not agree within 30°!! To expect to have any general law to connect and agree with experiments so unaccountably discordant would be absurd in the extreme. The best course which it appears to me can be taken in this case, is to select the experiments of a single author, and from one of them to determine the numeratoms, and then compare the theory with the rest; for it is highly probable, if he conducted all his experiments the same way, that any errors which had crept into one would proportionably run through the rest, and thence, in a similar way, affect the theory with all. Dr. Henry has indeed not told us whether the numbers he has given be the results of experiment or calculation; but from the way in which he has introduced them, I should think they were derived from experiment.

A beautiful case presents itself for ascertaining the truth of our theory to any one who chooses to undertake it. When water and mercury are mixed at a great interval of temperature, 180° for instance, the results, if our theory be true, by first putting the one body and then the other at the higher temperature, will unequally differ from the arithmetical mean of Fahrenheit. Thus, supposing the above ratio of the numeratoms to be correct, a volume of water at 212° mixed with an equal volume of mercury at 32° should give a temperature of 148·9°, and a volume of mercury at 212° mixed with an equal volume of water at 32° should produce a temperature of 88·8°. These numbers differ from the arithmetical mean 122° by 26·7° and 33·2°. A slight difference in the ratio of the numeratoms would but triflingly affect the discordance of these differences. It is manifest, therefore, if experiments can be correctly made with about this difference of temperature, the results will be sufficiently marked to decide the truth of our theory respecting the cause of the phænomena attributed to "Calorific Capacity." Were the present notions of capacities true, the results, if I understand correctly the doctrine, should be equidistant from the arithmetical mean. But to ascertain this inequality of distances, we are not obliged to use water and mercury; we may use water alone by substituting for the mercury half its volume of water, or mercury alone by substituting for the water double its volume of mercury.

If the same temperatures were used, 32° and 212° of Fahr. and water or mercury alone was used in quantities in the proportion of 6 to $6\frac{1}{9}$, one of the temperatures would come out 122° Fahrenheit's arithmetical mean, and the other no less than 7·3° below it. This inequality would be very striking; and as it is closely connected with the doctrine of capacities, it would alone, it appears to me, decide the fate of that hypothesis.

Prop. IV. Theor. III.

Denoting the volumes mixed by $V\ V_1$, and the numeratoms and true temperatures as before by $N\ N_1$ and $T\ T_1$, the true temperature (τ) of the mixture will be equal to $\frac{T\,V\,N + T_1\,V_1\,N_1}{V\,N + V_1\,N_1}$.

By the same train of reasoning as in the last Proposition, $\tau\,(V\,N + V_1\,N_1) = T\,V\,N + T_1\,N_1\,V$, and, therefore, $\tau = \frac{T\,V\,N + T_1\,V_1\,N_1}{V\,N + V_1\,N_1}$.

Cor. 1.—When $V = V_1$ the theorem reduces itself to the same as the last.

Cor. 2.—Taking $V_1 = v\,V$ and $N_1 = n\,N$, the general theorem becomes $\tau = \frac{T + v\,n\,T_1}{1 + v\,n}$; and, therefore, when the volume is so taken that $v\,n = 1$, or that $V\,N = V_1\,N_1$, $\tau = \frac{1}{2}(T + T_1)$; in which case, as we have remarked in the last Scholium, it is immaterial which of the fluids be put at the higher temperature, the result will be the same.

Cor. 3.—Because $\tau\,(V\,N + V_1\,N_1) = T\,V\,N + T_1\,V_1\,N_1$, we have $N : N_1 :: \tau\,V_1 - T_1\,V_1 : T\,V - \tau\,V$. Therefore, the volumes of two fluids being given, the temperatures at which they are mixed, and the temperature of the mixture, the ratio of the numeratoms may be found.

Cor. 4.—In most experiments, it is much easier to determine the proportion of the weights than of the volumes, in which case it will be better to have the ratio of the numbers of the particles in equal weights. Let P, P_1, denote the numbers of the particles in equal weights, and W, W_1, the weights themselves, then the number of particles in each fluid being as $W\,P$ or $W_1\,P_1$, it is evident that $\tau = \frac{T\,W\,P + T_1\,W_1\,P_1}{W\,P + W_1\,P_1}$.

Cor. 5.—From the preceding cor. it follows that $P : P_1 :: W_1\,(\tau - T_1) : W\,(T - \tau)$; and that if W be so taken, that $W\,P = W_1\,P_1$ $\tau = \frac{1}{2}(T + T_1)$ a case analogous to that of cor. 2.

PROP. V. PROB. II.

Two fluids being given, it is required to investigate the conditions of the mixture so that the theory may be examined under the most advantageous circumstances; that is, so that the distances of the resulting temperatures from Fahrenheit's arithmetical mean shall be the most unequal.

Since the equation $\tau = \frac{T\,W\,P + T_1\,W_1\,P_1}{W\,P + W_1\,P_1}$, found in Cor. 4 of the preceding Prop. is precisely the same as $\tau = \frac{T\,V\,N + T_1\,V_1\,N_1}{V\,N + V_1\,N_1}$ in the context of the theorem, by changing W into V, and P into N, it is plain we may in the present inquiry use either. We will, therefore, take that given in the corollary. Let us put $W_1 = n\,W$, and we have $\tau = \frac{T\,P + n\,T_1\,P_1}{P + n\,P_1}$. By Cor. 2, Prop. I. if we put $T_1^2 = F_1 + 448$, $T^2 = F + 448$, and $\tau^2 = F_{11} + 448$; therefore, $F_{11} = \left(\frac{T\,P + n\,T_1\,P_1}{P + n\,P_1}\right)^2 - 448$; and the other F_{11} equals $\left(\frac{T_1\,P + n\,T\,P_1}{P + n\,P_1}\right)^2 - 448$ by changing T_1 for T, and T for T_1, that is, by reversing the bodies with respect to the temperatures, or, which is the same, the temperatures with respect to the bodies. Moreover the Fahrenheit arithmetical mean is equal to $\frac{F_1 + F}{2} = \frac{T_1^2 + T^2}{2} - 448$. Hence $\frac{T_1^2 + T^2}{2} - \left(\frac{T\,P + n\,T_1\,P_1}{P + n\,P_1}\right)^2$ is the distance of one result from Fahrenheit's arithmetical mean, and $\left(\frac{T_1 P + n\,T\,P_1}{P + n\,P_1}\right)^2 - \frac{T_1^2 + T^2}{2}$ the distance of the other; and, therefore, $\left(\frac{T_1\,P + n\,T\,P_1}{P + n\,P_1}\right)^2 + \left(\frac{T\,P + n\,T_1\,P_1}{P + n\,P_1}\right)^2 - (T_1^2 + T^2)$ is the difference of these distances, which, to satisfy the object of the problem, must be a maximum. But the temperatures T, T_1, and the particles, P, P_1, being the same, n is the only variable. Whence we have $d\,.\left(\frac{T_1\,P + n\,T\,P_1}{P + n\,P_1}\right)^2 = -\,d\,.\left(\frac{T\,P + n\,T_1\,P_1}{P + n\,P_1}\right)^2$ and

$$\frac{T_1\,P + n\,T\,P_1}{P + n\,P_1}\,.\,d\left(\frac{T_1\,P + n\,T\,P_1}{P + n\,P_1}\right) = -\,\frac{T\,P + n\,T_1\,P_1}{P + n\,P_1}\,.\,d\left(\frac{T\,P + n\,T_1\,P_1}{P + n\,P_1}\right).$$

But omitting $d\,n$, which is common to both sides, $d\,\frac{T_1\,P + n\,T\,P_1}{P + n\,P_1}$

$$= \frac{T\,P_1\,(P + n\,P_1)}{(P + n\,P_1)^2} - \frac{P_1\,(T_1\,P + n\,T\,P_1)}{(P + n\,P_1)^2} = \frac{T P_1 P + n T P_1^2 - T_1 P_1 P - n T P_1^2}{(P + n\,P_1)^2}$$

$$= \frac{(T - T_1)\,P\,P_1}{(P + n\,P_1)^2}, \text{ and } d\,\frac{T\,P + n\,T_1\,P_1}{P + n\,P_1} = \frac{T_1\,P_1\,(P + n\,P_1)}{(P + n\,P_1)^2} -$$

$$\frac{P_1\,(T\,P + n\,T_1\,P_1)}{(P + n\,P_1)^2} = \frac{T_1\,P_1\,P + n\,T_1\,P_1^2 - T\,P_1\,P - n\,T_1\,P_1^2}{(P + n\,P_1)^2} = \frac{(T_1 - T)\,.\,P P_1}{(P + n\,P_1)^2}.$$

The two differential parts, therefore, in the above differential equation being the same quantities with contrary signs, and the sides of the equation itself having contrary signs, we have $\frac{T_1\,P + n\,T\,P_1}{P + n\,P_1} = \frac{T\,P + n\,T_1\,P_1}{P + n\,P_1}$; and consequently $n = \frac{(T_1 - T)\,.\,P}{(T_1 - T)\,.\,P_1} = \frac{P}{P_1}$. Restoring the value of n, that is $\frac{W_1}{W}$, we get $P_1 W_1 = P W$, and also $N_1\,V_1 = N\,V$. Consequently the most advantageous method of examining the theory is when the interval of the temperatures is the greatest possible, and the weights reciprocally proportional to the particles in equal weights, or the volumes reciprocally proportional to the numeratoms. That is, the temperatures being the same, the best method to make the experiment, or to examine the theory, is when the weights of the fluids are so related to the particles in equal weights, or the volumes to the numeratoms, that $\tau = \frac{1}{2}\,(T + T_1)$, or that the temperatures are symmetrical with respect to themselves and the fluids.

Cor 1.—When $P_1 = P$ or $N_1 = N$, $W_1 = W$ and $V_1 = V$; so that in parts of the same fluid the most advantageous circumstance under which the theory can be examined, is when the portions mixed are equal; which coincides with what we have deduced in Prop. II.

It is proper to remark, that in this deduction one of the distances from the Fahrenheit arithmetical mean becomes negative with respect to the other; so that the true difference of distances is double that found by experiment. Without attending to this remark, we should find that the theory would differ most from Fahrenheit's when the volumes or weights are so taken, that one of the results coincides with Fahrenheit's arithmetical mean. Either case of this condition is obtained by taking $\frac{W_1}{W} = \frac{T - \sqrt{\frac{T^2 + T_1^2}{2}}}{\sqrt{\frac{T^2 + T_1^2}{2}} - T_1} \cdot \frac{P}{P_1}$ or $\frac{V_1}{V} = \frac{T - \sqrt{\frac{T^2 + T_1^2}{2}}}{\sqrt{\frac{T^2 + T_1^2}{2}} - T_1} \cdot \frac{N}{N_1}$.

Cor. 2.—It also follows that if Q be any true temperature, the ratio of the volumes or weights to produce that temperature are obtained from the equations $\frac{V_1}{V} = \frac{T - Q}{Q - T_1} \cdot \frac{N}{N_1}$, and $\frac{W_1}{W} = \frac{T - Q}{Q - T} \cdot \frac{P}{P_1}$.

PROP. VI. THEOR. IV.

The ratio of the numeratoms and specific gravities of two homogeneous bodies being given, the ratio of the mass of a particle of the one body to that of a particle of the other is compounded of the direct ratio of the specific gravities and the reciprocal of the numeratoms.

In page 411 of the last volume of the *Annals*, I have shown from my theory of gravitation that the weights of any two spherical bodies towards a third, at equal distances, are directly as their quantities of matter; and, consequently, the weights of any two bodies towards a third at such a distance from this third that their figures do not interfere with the sums of the gravitating weights of all their parts, are directly as those sums; that is, as the quantities of matter in the whole bodies. But the weights are as the volumes, and what we call the specific gravities conjointly; and the quantities of matter are likewise as the individual masses of the particles, numeratoms, and volumes, conjointly. The ratio, therefore, of the mass of a particle of the one to the mass of a particle of the other in any two homogeneous bodies, when the numeratoms are equal, is equal to the direct ratio of the specific gravities of the bodies; and when the specific gravities are equal, to the inverse ratio of the numeratoms. But when neither specific gravities nor numeratoms are equal, the ratio of the masses of the two particles is equal to that compounded of the simple of the specific gravities and the reciprocal of the numeratoms.

Cor.—Because there is every reason to believe that the ultimate atoms of all bodies are composed of the same kind of matter, the magnitudes of the constituent particles of any bodies, if similarly composed of pores and solid parts, will have the same ratio as the masses of the particles. The one ratio, therefore, being known, the other becomes known; and, consequently, if the figures of the particles are similar, the ratio of their diameters becomes known, which is the subtriplicate of the ratio of the magnitudes.

It is to be observed that it is not necessary for the equality of the ratios of the magnitudes and masses of the particles, that the particles be composed of similar atoms similarly arranged; it is enough if the quantity of solid matter to the quantity of pores in each particle has the same ratio.

Scholium.

Mercury, according to the best experiments I can meet with, has a specific gravity about 13·5 times greater than that of distilled water; therefore, the mass of a particle of mercury is to the mass of a particle of water, supposing both homogeneous fluids, and that the experiments related by Dr. Henry are right, in a ratio compounded of that of 1 to 13·5, and that of 1 to 2, or in a ratio equal to that of 1 to 27. A particle of mercury consequently contains 27 times more solid matter than a particle of water; and, if the conditions in the preceding corollary are fulfilled, is 27 times greater in magnitude, and three times greater in diameter, which agrees with what I have stated p. 406 of the last volume of the *Annals*.

That water has really more particles in a given space than mercury, and that it is to this its supposed superior capacity for heat is owing, may be made evident from our principles in a general way thus. Water and mercury mixed in equal volumes always produce a temperature nearer to that of the water than to the temperature of mercury. But by our theory of heat the temperature of a body is measured by the intensity of the motion or vibration of one of its particles. When, therefore, portions of two bodies at unequal temperatures are mixed together, the temperature or mean motion of a particle of the mixture will deviate less from the temperature or individual mean motion of the greater number of particles than from that of the less; and consequently when equal volumes of two bodies are mixed that body will have the greatest number of particles from whose temperature the temperature of the mixture deviates the least. Hence, therefore, water has a greater number of particles than mercury.

Now if mercury has a less numeratom than water, and the specific gravity of mercury be greater than that of water, the mass of a particle of mercury must exceed that of a particle of water.

This last inference might be demonstrated in a different way by other principles, which will at the same time presents us with an elegant and a beautiful illustration of the coincidence and agreement of apparently the most independent and unconnected parts of our general theory of the universe. I shall at present merely touch on the subject in a general way, and shall not stop to enter into it mathematically, because at some future period it may come forth connected with investigations of a much more general and abstruse nature. According to our theory of heat, it is the motion or momentum of each particle of a body, and not the velocity which measures the temperature of the body. A particle, therefore, which is greater than another indicating the same temperature, will have a less velocity in the inverse proportion of its mass to the mass of the other particle. But by what I have shown in p. 408 of the last volume of the *Annals*, the tendency of one spherical particle towards another is, *cæteris paribus*, as its mass; the greater the particles, therefore, of any body, the greater will be their mutual cohesive tendency. Consequently, if other things be nearly alike, and one body be composed of greater particles than another, the particles of that body at the same temperature as the other will not only have a less vibratory velocity, but will have a greater cohesive tendency; on both of which accounts the maximum range of separation of the particles, or, which is the same, the expansion of the body due to the temperature, will be less in the body with the greater particles than in an equal volume of the body with the less particles. And because this is the case for any common temperature, it is also the case for any common increment or decrement of temperature; and, therefore, the greater the particles of any body, the less will be the expansion or contraction of a given volume of that body for a given increment or decrement of temperature. Hence the expansion of mercury being less than that of water for a given increase of temperature, the particles of mercury are greater than those of water.

From these considerations, we infer that bodies which are the most expansible by heat have in general the greatest numeratom. For as the greater expansibility is an argument of a less magnitude in the particles, so the inferiority in magnitude is an argument of the excess in number in a given space. This rule, as well as the preceding, is not, however, to be considered as universal. Philosophers will not expect where there is so great a variety of formation and constitution as in the numerous bodies we are acquainted with, any thing in the shape of universality. That we have approached in this general inference pretty near the truth may be gathered from the following observations deduced from phænomena in "Davy's Elements of Chemical Philosophy," p. 77. "In general," says Sir H. "it appears that the substances most expansible by heat are those which have the greatest capacities. Thus gases in general have greater capacities than fluids, and fluids than solids; but the exact ratio has not yet been determined." This greater capacity in its implied sense evidently coincides with our greater numeratom; but it is by no means a general truth that the numeratom of gases is greater than that of solids and fluids; in fact, the contrary is the case. Sir H. has here manifestly before him the capacities as referred to equality of weight. In this sense aeriform bodies have generally a much greater influence on temperatures than solids and fluids. For instance, if a given weight of any body be immersed in a given weight of any gas or air at a given difference of temperature, the effect on the temperature of the body will be much greater than if an equal weight of any solid or fluid with the same difference of temperature had been mixed with the body, taking for granted, in both cases, that no chemical action interferes. The reason of this will appear in the course of our subsequent inquiries. In the mean time, however, it may be anticipated, that even with aëriform and solid or fluid bodies, there is no reason for believing it a universal, though it be a pretty general rule, that the number of particles in a given weight of air shall exceed the number of particles in an equal weight of the solid or fluid from which it is derived. Thus in all those bodies which explode with an increase of temperature, if there be no solid or fluid residuum, I have no doubt experiment would prove that what is called the capacity is less in the air than in an equal weight of the solid or fluid. We shall, however, as we proceed, have to develop our reasons for this more fully.

We are now prepared, by the help of the principles here developed and those given in my last paper, to carry our inquiries to a much more interesting and extended length; for as the numeratoms of gaseous bodies may be determined by ascertaining their "capacity," and since the ratio of these numeratoms may also be found by our theory, p. 403 of the last volume of the *Annals*, on the supposition of the homogeneity of the gases from their specific gravities, we can, by comparing these ratios, ascertain the relative purity or impurity, or rather the relative homogeneity or unhomogeneity of the gases. And if we again compare these numeratoms of the constituent gases with the numeratoms of the body they compose in the vapourous, fluid, and solid states, determined in the manner hereafter described, we may penetrate into the inmost recesses and operations of nature; we may unfold to the view the relative proportions of the elementary parts to form a particle of the solid, and likewise a particle of the fluid and vapour; and again, by comparing those with the like things of the same elements of another body, and with other phænomena of the two bodies, as their relative degrees of compactness, affinity, chemical effects, &c. it does not seem impossible even to ascertain the very figures of the constituent particles and atoms. Of such extensive and recondite inquiries, however, philosophers will hardly expect of me in the present instance any thing more than the hint. From those who are better qualified for speculations of this kind, the world may hereafter derive investigations suitable to the utility and importance of the subject, and supported by all the evidence and rigour of mathematical demonstration.

Before I quit this part of the subject, it is necessary just to mention a circumstance which seems to confirm our general conclusion, though it must be confessed that the fundamental experiment was of too delicate a nature to elicit an implicit confidence. Crawford found that the "capacities" of equal weights of oxygen and hydrogen have a ratio of about 1 to 4·5. By our theory, the numeratoms of oxygen and hydrogen on the supposition of their homogeneity are (p. 403 of the last volume of the *Annals*) as 4 to 1; and, therefore, the numbers of atoms, or Crawford's capacities of equal weights, are as 1 to 4. From the near agreement of these ratios, we should be led to infer, that oxygen and hydrogen are homogeneous gases; and if they are, the experiment and theory may be esteemed a mutual confirmation of each other. A similar confirmation of the general truth of our theory may be found in some of the other gases; but until some more unexceptionable experiments on the "capacity" of the gases are obtained, it will be useless to pursue a numerical comparison. One thing, perhaps, the attentive reader will observe in these deductions very opposite to what I anticipated in p. 403 of the last volume of the *Annals,* namely, that if the experiments can be correctly made, we may, by direct experiments, ascertain the relative number of particles in any two gases; for the proportion of the "capacities" applied to our temperature, will give the proportion of the numeratoms of equal weights whether the gases be homogeneous or not.

Having extended our theory of the mixture of bodies which do not act chemically on each other as far as I think it needful, it may be expected that I should enter into a critical investigation of the accuracy of the doctrine of "capacities," and show the experiments by which it may be directly refuted. Such things are by no means difficult to do, but as at the end of this paper I have some idea of examining the soundness of our present doctrine of "Latent Heat," I shall reserve any observations I may have to offer until then; and, if it should appear necessary, put them out together.

I might now extend my inquiries to the mixtures, numeratoms, &c. of three and more bodies; but as these things easily flow from what has been said of two bodies, it would be more an object of curiosity than utility to carry the investigations any further. Before I attempt to draw the attention of philosophers to the simplicity and fecundity of this theory, I shall proceed to a development of the phænomena attributed to the celebrated doctrine of "Latent Heat."

(*To be continued.*)

Article VIII.

On a Method of expressing Chemical Compounds by Algebraic Characters. By Mr. Charles Sylvester.

(To the Editor of the *Annals of Philosophy.*)

MY DEAR SIR, 60, *Great Russel-street.*

I have often, in conversation with you, mentioned the want of some easy and simple mode of expressing chemical compounds, at once showing their elementary constituents. It is well known to mathematicians, that if the relations of quantities under various circumstances had to be expressed by common language, it would be a task so laborious as to render most of the operations of analysis impracticable. The notation which I would adopt for the expression of chemical compounds is precisely that employed in algebra, excepting that I would use none of the signs but that of equality, and that should be used only to express the equality of all the elements before and after decomposition.

The weights of the atoms of the different simple bodies I would represent by letters of the alphabet, not permanently fixed for each, but assumed discretionally, as is the case in algebra, stating beforehand what letters shall be used for each elementary substance in comparing the bodies which are the subject of examination. These letters I would use to express the compounds precisely as in alegebraic products, by placing them together as in forming a word, when the same letter would be repeated in a compound, which would be the case when more than one atom is combined, I would use an exponent, which is, a small figure placed above the letter a little to the right side expressing the number of atoms; as, for instance, if a and b were to represent an atom each of a body, the compound would be expressed by ab; but if two atoms of a had to combine with b, then a^2b will express such a compound.

To make this a little more familiar, we will assume a to be azote, c carbon, o oxygen, h hydrogen, p potassium. Then we shall have ao nitrous oxide, ao^2 nitrous gas, ao^4 nitrous acid, and ao^5 nitric acid. In the same way co is carbonic oxide, and co^2 carbonic acid. The whole of the compounds of these elements will be as follows:

ao nitrous acid,
ao^2 nitrous gas,
ao^4 nitrous acid,
ao^5 nitric acid,
(ao^5) $(\mathrm{P}o)$ nitrate potash,

(ao^5) (ah^3) nitrate ammonia,
(co^2) (Po) carbonate potash,
(co^2) (ah^3) carbonate ammonia,
$(co^2)^2$ (Po) bicarbonate of potash,*
ho water,
ah^3 ammonia,
po potash,
co carbonic oxide,
co^2 carbonic acid,
ch olefiant gas,
ch^2 carburetted hydrogen.

In order to give some idea of the facility which this notion affords when chemical decompositions take place, we will give a few examples.

The decomposition of nitrate of ammonia by heat used to obtain the nitrous oxide would be expressed as follows: (ao^5) $(ah^3) = (ao)^2$ $(ho)^3$, which is two atoms of nitrous oxide, and three atoms of water. At one view, it will be seen that the elements are the same on each side the sign of equality; and if numbers be substituted for the letters, add them together on each side, and the sums will be equal. In instances where mutual decomposition takes place, the sign of equality is made to separate the quantities before and after decomposition. In the action of chlorine upon a solution of potash, let C = chlorine. Then C^5 $(ho)^4$ $(po)^5 = (Ch\ po)^4$ $(Co^5\ p)$ = four atoms of muriate of potash, and one of chlorate of potash.

When chemical changes take place among gaseous bodies, it is common to state their proportions by volumes. This will be easily managed when the specific gravity of the gases are known. This can be ascertained by experiment; but some very curious facts have lately come to light, showing a remarkable connection between the specific gravity of a gas and the weight of its atom. When hydrogen is made unity, the weight of the atom of a gas is either equal to the specific gravity, or some multiple of the same by a whole number.†

The following table will show the weight of the atom and the specific gravity:

	Atoms.	Sp. Gr.
Oxygen	o	o^2
Hydrogen	h	h
Azote	a	a
Chlorine	C	C

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* This parenthesis is used to distinguish the acid from the base; and when more than one atom of acid occurs, an exponent would be used. Indeed, where no exponent is used, it must be conceived equal to 1. Then $a = a^1$ and $(co^2) = (co^2)^1$.

† The relation of the specific gravity to the weight of the atom I pointed out to Mr. Dalton, Dr. Henry, and to Dr. Thomson, long before the account of it was published by Dr. Prout in Thomson's *Annals*.

	Atoms.	Sp. Gr.
Carbon	c	....
Sulphur	s	....
Phosphorus	f	....
Steam	oh	oh
Nitric oxide	ao	ao
Nitrous gas	ao^2	$(ao^2)^{\frac{1}{2}}$
Muriatic acid	Ch	$(Ch)^{\frac{1}{2}}$
Carbonic oxide	co	co
Carbonic acid	co^2	co^2
Sulphuret hydrogen	sh	sh
Phosphuretted hydrogen	Fh	Fh
Ammonia	ah^3	$(ah^3)^{\frac{1}{2}}$
Olefiant gas	ch	$(ch)^2$
Carburetted hydrogen	ch^2	ch^2

If the ratio by weight of two gases, which act upon each other, be known, the ratio of their volumes may be known by multiplying the ratio of their weights by the inverted ratio of their specific gravities.

In forming water, the ratio by weight will be $\frac{o}{h}$, and the ratio of their specific gravities will be $\frac{o^2}{h}$, and inverted is $\frac{h}{o^2}$. Hence $\frac{o}{h} \times \frac{h}{o^2} = \frac{1}{2}$, which is one volume of oxygen to two volumes of hydrogen. It will be seen that in reducing the above ratios, it is only necessary to take away the letters, and leave the exponents; as in the above $\frac{o^1}{h^1} \times \frac{h^1}{o^2} = \frac{1}{1} \times \frac{1}{2} = \frac{1}{2}$ as before.

The ratio by weight of oxygen to carbonic oxide will be $\frac{o}{co}$, or o to co. The inverted ratio of their specific gravities will be $\frac{co}{o^2}$. Then $\frac{o}{co} \times \frac{co}{o^2} = \frac{1}{1} \times \frac{1}{2} =$ one volume of oxygen to two of carbonic oxide. The ratio of oxygen to olefiant gas is $\frac{o^3}{ch}$. Then $\frac{o^3}{ch} \times \frac{(ch)^2}{o^2} = \frac{3}{1} \times \frac{2}{2} =$ three volumes oxygen to one olefiant gas.

Oxygen to carburetted hydrogen $= \frac{o^4}{ch^2}$. Then $\frac{o^4}{ch^2} \times \frac{ch^2}{o^2} = \frac{4}{2} \times \frac{2}{2} = \frac{2}{1}$, which is two volumes oxygen to one of carburetted hydrogen. In order to see what quantity of oxygen will be required for the saturation of an inflammable gas, write down the gas first, as in olefiant gas, ch; then it will be seen that c the carbon will want two atoms of oxygen, and h the hydrogen one atom: hence it will be $\frac{o^3}{ch}$ for the ratio by weight. In the same way ch^2 carburetted hydrogen will want two for the carbon and two for the hydrogen = four, making $\frac{o^4}{ch^2}$.

In some chemical investigations, it is found necessary to have recourse to real algebra; in which case the same means may be employed to represent the atoms and volumes. An instance of this kind occurred to me in reading a very excellent paper by Dr. Henry, upon the gases used for illumination, read before the Royal Society, Feb. 22, 1821.

After, by his very elegant process, he has separated the olefiant gas, by means of chlorine, he states that the proportions of the residual gases have to be estimated from the joint data of the quantity of oxygen required to saturate the gas, the quantity of carbonic acid produced, and the specific gravity found by experiment, which was ·534, common air, being 1, or unity. In examining this method I found that the datum of the sp. gr. is not necessary to find the proportions of the remaining gases, which are carburetted hydrogen, free hydrogen, and carbonic oxide. The oxygen which 100 volumes of the mixed gas required was 110 volumes, and the carbonic acid produced was 70 volumes.

Now let x = volumes of carburetted hydrogen in 100 volumes of mixed gas; y = the hydrogen; and z = the carbonic oxide. Then $x + y + z = 100$ the quantity employed.

Then since x will take $2\,x$ volumes of oxygen, y $\frac{1}{2}y$ volume, and z $\frac{1}{2}z$ volumes, we shall have $2\,x + \frac{1}{2}y + \frac{1}{2}z = 100$. The carbonic acid produced by x will be equal to x, and that by z equal to z.

Therefore $x + z = 70$.

Hence we get the three following equations:

$$\begin{aligned} x + y + z &= 100 \\ 2\,x + \tfrac{1}{2}y + \tfrac{1}{2}z &= 110 \\ x + z &= 70 \end{aligned}$$

By subtracting the third from the first, we get,

$$y = 30$$

The second, multiplied by 2, gives,

$$\begin{array}{r} 4\,x + y + z = 220. \\ x + y + z = 100 \\ \hline \text{This gives } 3\,x = 120 \end{array}$$

From this subtract the first,

Hence $x = 40$. And since $z + 40 + 30 = 100$.

Therefore $z = 30$.

These numbers are the same with Dr. Henry's.

If the datum of the specific gravity be employed, one of the others may be left out. In this case, the specific gravity being taken at ·534, will not give the result exactly as above. Hence the above formula becomes a means of finding the exact specific gravity, which will be ·5347. I am, yours truly,

C. Sylvester.

Article IX.

On the Geological Formations of Headen Hill, in the Isle of Wight. By G. B. Sowerby, FLS.

(To the Editor of the *Annals of Philosophy.*)

SIR,

The combined efforts and abilities of a most respectable and learned Society have been exerted for some years in the cause of geological science; their attention has been directed to the careful examination of various interesting districts; facts of vast importance have been ascertained, and the volumes of their Transactions are stored with considerable and useful information upon the various points in connexion with, and generally leading to the discovery of, truth in their pursuit. Notwithstanding, however, the zeal by which their exertions have been stimulated, and the great degree of labour with which they have been accompanied, it will, I think, appear to any reflecting mind, that their success, and the consequent increase of geological knowledge, is not proportionably great. We are but just arrived at that stage in which we begin to discover the existence, and to estimate the extent, of our ignorance—ignorance rather the result of wilful negligence than of unavoidable necessity. In support of this opinion, I will appeal to that reply which must be given to the following plain question: How can any person know the nature of any stratum, until he have taken the pains to make himself acquainted with its constituent parts? For instance, can any person decide upon the nature of a mass of granite until he know how to distinguish among minerals the three substances of which it is composed? How can any person decide confidently on the nature of a shelly stratum, unless he know whether the shells which characterize it be land, or freshwater, or marine? The undisputed importance of geological investigation renders it extremely desirable to collect the most undoubted evidence of facts as they are, before we can hope to arrive at any thing like certainty in the conclusions we may be disposed to draw from them. Every particle of information that can be gained is, therefore, of some consequence. These are my reasons for troubling you with the following observations collected during a short examination of Headen Hill, in the Isle of Wight, which, I regret, it has been impossible for me to complete owing to the very contracted period it was in my power to allot to the purpose. This spot, it is well known, has been repeatedly examined; a description of it forms the principal part of a memoir published in the second volume of the Transactions of the Geological Society. The particular object of my visit to it was to collect the fossil freshwater shells that abound there, for the illustration of that part of the subject in the admirable work on Land and Freshwater Mollusca now publishing by De Ferussac. I wished also to obtain a regular series of the strata above the chalk, in the uppermost of which exist such multitudes of freshwater fossils. Of course, I took Mr. Webster's memoir, abovementioned, with me; I was to be guided by it. I, therefore, trust I shall be acquitted of having gone there with an intention to criticize its respectable and learned author; if, entertaining difference of opinion, though founded upon the examination of the same spot of ground, and the same kinds of organized fossils, I should find myself compelled to express my dissent from what he has advanced; following the strata in the same order; but as regards the nature of one particular stratum, guided by a more intimate conchological knowledge.

One or two general observations shall suffice for the "lowest marine formation above the chalk, including the plastic clay and sand, together with the London clay." The present state of the vertical cliffs is such as renders it quite impossible to trace the sand and clay in the order in which Mr. W. has described them. Indeed these cliffs appear to consist rather of vertical *beds* than of continuous strata; and must, therefore, be as constantly varying as the weather and other natural causes operate to produce changes in the form of the cliffs. Every thing is in favour of the opinion, that from the chalk to the lowest part of the freshwater stratum, the whole is but *one formation*, consisting of various beds of sand and clay. Indeed Mr. W. says, "upon reviewing the whole of this lower marine series of strata in Alum Bay, and comparing it with other sections of the strata immediately over the chalk, we shall find it useful, for the present at least, to separate it into *two* great divisions: 1. Sand and plastic clay. 2. London clay. From the irregularities in the beds in the few places where there are good sections, these divisions, however, can as yet scarcely be considered as distinctly determined;" but he goes on to say, "thus much is certain, that the plastic clay and sand are always *below*, and never *above*, the London clay." But it appears to me that he has himself described a bed of London clay, which he marks *d*, in Plate XI. of vol. ii. Geol. Trans. very near to the chalk, and placed below the greater part of the beds of plastic clay and sand, which are again surmounted by the bed of London clay marked B in the same plate. It is true he advances an opinion that the bed marked *d* is not continuous; and he speaks of the nodules contained in it as "*limestone,*" as if he did not think it identical with the London clay. But here I must object, first, that there is just as much *apparent* continuity in that bed of clay as in the one marked B; secondly, that the nodules in it are exactly of the same nature as those he calls "septaria" in the stratum B; thirdly, that the fossils found in these septaria in stratum *d* are identical with those constantly found in the London clay almost wherever it occurs.

I beg leave to state one more fact connected with this "*lowest* * marine formation above the chalk," the singularly disintegrated siliceous pebbles found in the white sand marked *q* †: also occur in the white sand D, which, according to Mr. W. forms the bottom of Totland and Colwell Bays—a fact further confirmative of the identity of the sand and plastic clay with the London clay. I may also add that similar pebbles have been taken up, attached to septaria at Highgate.

The next stratum upon which I shall trouble you with some observations is that announced by Mr. W. as an "upper marine formation," which, he says, "contains a vast number of fossil shells *wholly marine*." Though the evidence which I must here produce is not sufficient to prove this formation to be *not marine*, yet it will go very far towards producing the conviction that it is not *wholly marine*, and evince the probability of its being a *freshwater* formation, or, at least, the produce of an estuary, in which, owing to some peculiar vicissitudes, some marine productions have become mixed with those of freshwater.

It appears proper for me to begin by observing, that notwithstanding Mr. W. has said of this stratum, that it contains wholly marine shells; yet in the list of those shells, he mentions five species, though two of them with doubt, of three genera, which are known to exist in a recent state only in freshwater; viz. *Cyclas*, 1; *Ampullaria*, 2; and *Melania*, 2. He mentions, but also with doubt, a genus of which all the recent species known, are land shells, viz. *Helicina*. Of the remaining species which he mentions as being found in that stratum, I will not presume to say absolutely that the following do not occur, but, I think, some of them are wrongly named, and others I do not believe are to be found in it. Cerithium lapidum or lapidorum; ancilla buccinoides; murex reticulatus; natica caurena. There exist in it, however, in great profusion, a species very much resembling cerithium lapidum; but this, with the remaining six species, which he gives under the generic name cerithium, may very probably be freshwater shells. Their recent analogues are found in the freshwaters of the Islands of Bourbon, Guadaloupe,

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* I think the following observations will prove this to be, strictly speaking, the *only marine* formation above the chalk at Headen Hill and Alum Bay.

† Whether this sand have changed its place since Mr. W. described it, or whether by some accident or mistake he placed it wrongly, I know not; but it is now seen to the N. of the most beautifully coloured bed of sand and clay.

Madagascar, the River Congo, &c. There is also found in great profusion a species of freshwater shell very like one known by the name of melanopsis buccinoides, and which, I think, Mr. W. may have mistaken for ancilla buccinoides. The only shell I can find in this stratum which at all resembles murex reticulatus is a small but beautiful species of melania. I have mentioned above my doubts about natica caurena being found in this stratum. I think a true freshwater nerita nearly related to Virginea may have been mistaken for it. It is remarkable that this nerita retains its colours almost as strongly as the recent shells of North Virginea; it is very abundant, and with the melanopsis abovementioned, and some others, it is found in the blackest coal-like layers of the formation, the shells in which are almost always in a very tender state, and fall to pieces on the slightest touch. I do not know the shell which Mr. W. calls cytherea scutellaria; neither am I acquainted with his "bivalve, apparently of the genus erycina," or his "murex nodularius." The cytherea and the erycina may possibly be cyclades, or cyrenæ, and the murex may prove to be a melania. Of these, I cannot say any thing certainly, not having met with them. The few marine shells I found were principally oysters; a species of buccinum, voluta spinosa, and ancilla subulata; the last three very sparingly indeed. Of these I should have found it more difficult to dispose, had not Mr. W. informed me that "occasional mixture of marine and freshwater shells may be expected," and that "they would denote either the gradual nature of the change that has taken place in an arm of the sea before it became completely a lake of freshwater, or the occasional irruptions of the ocean at a subsequent period." I must, however, observe, that the oysters are only found here and there in small patches, and that they are not only mixed with those of whose freshwater nature it is difficult to speak decidedly, but with *lymnæi* and *paludinæ*, which Mr. W. has not noticed in this stratum, though, I believe, he has noticed the same species of paludina, under the term cyclostoma, in the "freshwater formations." This is very abundant in some parts of this stratum, and is accompanied, also, by the shell which I suppose to be potamides Lamarckii,* one of the commonest in the stratum.

I think I have now nearly proved this stratum to be rather of freshwater than of marine origin. Thus much is certain, that by far the greater number of shells found in this stratum are freshwater, and *not* marine; so that "if we depend upon fossils as a principal means of identifying strata," we shall see great reason to believe *that there does not exist* any marine formation between the two freshwater ones, which Mr. W. designates as such; consequently that there are only known at Headen Hill, or Alum Bay, two distinct formations above the chalk; first, the sand and plastic clay, including the London clay; and second, the freshwater formation, consisting of several beds varied in their contents.

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* I may be mistaken about the name of this shell, because I have no means of ascertaining it with certainty, Mr. W. not having referred to any description of it. I conjecture it to be so from its resemblance to a cerithium, and from its possessing the characters of some shells which I know to be inhabitants of freshwater, and also resembling cerithia.

There is some reason to believe that the Woolwich beds may be cotemporaneous with this "upper marine formation;" for many of the shells contained in it are species of freshwater genera. That of Plumsted is much more evidently a marine formation, if we are to form our judgment from the shells it contains; but I cannot see any reason for supposing the "crag" to be identical with either. All the fossils I recollect to have ever seen from it are decidedly marine, and the formation bears evident marks of identity with alluvium.

I shall close these observations with two upon the shells contained in a bed of clay, 11 feet in thickness, placed over Mr. W.'s upper freshwater formation, and which, he says, are unmixed with any other species, and of such a singular character,' that Mr. Parkinson could not refer it to any known genus. First, I cannot say it is absolutely mixed with any other species here; yet there is within *a foot* and *above* it a bed of ironstone, two inches thick, on both sides of which are immense quantities of the same paludina as is found mixed with lymnæi in some parts of Mr. W.'s upper marine. Secondly, in generic characters, the small bivalve shell in question very nearly resembles corbula; but, though the hinge cartilage is internal, and the two valves unequal, as in that genus, yet there are some differences, and there is strong evidence of its freshwater origin about the umbones which are eroded. Its recent analogue is described under the name myalabiata, and figured from the Rio de la Plata in the Transactions of Linnæan Society, vol. x. to xxiv. f. 1, 2, 3, p. 326. Yours, &c.

G. B. Sowerby.

Article X.

On Arragonite. By the Rev. Dr. Daubeny.

(To the Editor of the *Annals of Philosophy.*)

Sir, *Magdalen College, Oxford, July* 7, 1821.

Dr. Clarke, of Cambridge, in his paper on Arragonite, published in the last number of the *Annals of Philosophy*, remarks, that "although Kirwan, 27 years ago, conjectured that this mineral contained strontian, and Prof. Stromeyer, of Gottingen, discovered strontian in some of the sub-varieties, yet it still remains to be proved whether this earth be an essential, or only a casual ingredient."

I imagine, from the above sentence, as well as from the silence of the journals which I am in the habit of perusing, that the recent experiments of Professor Stromeyer on this subject are not generally known in Great Britain; and, under this impression, I may just mention, that having visited Gottingen last summer, in the course of a tour through the north of Germany, I had some conversation with Prof. Stromeyer on this and similar subjects, and was informed by him that he had lately analyzed no less than 18 varieties of arragonite, several of them carefully selected from the very localities with those which, under the hands of other experimenters, had afforded results so opposite; and that all, with *one* exception, had yielded traces of strontian in a greater or less proportion.

This solitary exception was the "coralloidal variety," or "flos ferri," in which no portion of the earth in question could be detected—a fact which may be alledged in corroboration of the argument adduced by Dr. Clarke to the same effect, from the circumstance of this mineral appearing to result from a simultaneous process with that by which calcareous alabaster (a mineral totally devoid of strontian) is deposited.

Professor Stromeyer, therefore, I believe, proposes to separate *this* from the crystallized varieties of arragonite, as the latter, according to him, not only contain strontian as a constant ingredient, but even have their *form* affected by the greater or less proportion in which this ingredient enters into their composition. Nor does it appear impossible that so small a portion of strontian should determine the crystallization of the mass, when we consider how inconsiderable a quantity of carbonate of lime will, in some instances, give its *form* to the aggregate, of which it forms a part, as in the instance of the Fontainbleau sandstone.

Without, however, pretending absolutely to decide whether the discovery of strontian be sufficient to remove this anomaly to the general law of the correspondence of crystalline form with chemical composition, it must, I think, be admitted, that unless I am incorrect in considering the authority of Prof. Stromeyer in those departments of Chemistry, to which he has devoted his principal attention, of equal weight with that of Thenard, Fourcroy, and Vauquelin, who have contradicted him, the positive assertion of this eminent chemist must, according to the common laws of evidence, outweigh the negative testimony of the *latter*; and if such experimenters as those above alluded to, be allowed to have been mistaken, it is no disparagement of the analytical skill of Mr. Holme, to whom Dr. Clarke afterwards alludes, to conceive that he may have also fallen into a similar error.

I should add that Prof. Stromeyer presented me with a paper containing the results of his principal analyses of arragonite, which I had intended sending to some scientific journal on my return to England; but as it has been unluckily mislaid, I must rely on the candour of your readers to receive these facts on my word, with such allowances as the circumstance of my stating them upon recollection, and not from notes, naturally call for, although I am pretty confident as to the accuracy of the more material parts of the information.

I have the honour to be, &c. &c.

Charles Daubeny.

Article XI.

On a Crystalline Sublimate from Oil of Cinnamon. By Mr. J. A. Turner.

(To the Editor of the *Annals of Philosophy.*)

SIR, *Norwich, July* 9, 1821.

Some time since I observed on the sides of a bottle, about a quarter full of oil of cinnamon, a curious crystalline appearance; and as I have not been able to find any account of a similar circumstance, I was induced to think it might, perhaps, be worthy a place in your *Annals.*

The crystals, which were needleform, were arranged in a most beautiful arborescent manner. Light appeared to influence their deposition, as the sides next the wall against which the bottle stood had only faint delineations.

The quantity of crystals I was able to obtain was very small (not more than a grain). They possessed the properties of camphor; they had the cetaceous feel of camphor between the teeth; the taste I could not distinguish on account of the oil which adhered to them, and from which (in consequence of the smallness of the quantity) I was unable to free them. Alcohol dissolved them; the solution evaporated spontaneously, left behind a white powder, which was redissolved in alcohol; and, by the addition of water, a precipitate was formed.

The cause of its volatilizing and crystallizing on the sides of the bottle, beginning at the very surface of the oil, still remains to be accounted for. Should you be able to explain it, you will much oblige

Yours respectfully,

John A. Turner.

ARTICLE XII.

Astronomical Observations, Aug. 1821.
By Col. Beaufoy, FRS.

Bushey Heath, near Stanmore.

Latitude 51° 37′ 44·3″ North. Longitude West in time 1′ 20·93″.

Aug. 4.	Immersion of Jupiter's second satellite		11h	04′	31″	Mean Time at Bushey.
			11	05	52	Mean Time at Greenwich.
Aug. 11.	Immersion of Jupiter's second satellite		13	42	25	Mean Time at Bushey.
			13	43	45	Mean Time at Greenwich.
Aug. 14.	Occultation of λ Aquarius by the moon	Immersion	8	48	17·8	Mean Time at Bushey.
		Emersion	9	37	16·0	

The immersion of λ was instantaneous, and the time certain to one second. Dew having rendered the object glass of the telescope somewhat obscure, the emersion was not so accurately determined. The observation of the immersion confirms my former opinion, that the moon is destitute of an atmosphere, or, if surrounded by one, it is very dissimilar to the terrestrial.

Aug. 18.	Immersion of Jupiter's first satellite	11h	58′	53″	Mean Time at Bushey.
		12	0	14	Mean Time at Greenwich.

ARTICLE XIII.

Remarks upon Mr. Herapath's Theory.

(To the Editor of the *Annals of Philosophy.*)

SIR,

In offering a few remarks on the very ingenious theory proposed by Mr. Herapath in some late numbers of the *Annals,* I beg to disclaim all wish for controversy, and to profess myself actuated by no motives but the desire of contributing to the advance of truth; and, therefore, trust that both Mr. Herapath and other readers will regard these animadversions with candour; and whether they shall be found to possess any claims to attention or not, will view them, at least, with impartiality.

The only part of his investigations which I shall at present make the subject of observation is that which relates to some particulars in the physical properties of gases, and the theory of heat.

In Prop. VIII. Mr. H. endeavours to show, that supposing in two portions of the same gas the numeratoms are equal, the elasticities will be as the squares of the temperatures. The principal step on which his proof depends is, that the elasticity varies as the momentum of the particles × the number of returns. We may certainly grant that the elasticity varies as the action of the particles against a given portion of the surface containing he gas, but it may fairly be questioned whether this action can be measured by the momentum × the number of returns. The momentum must be the mass × the velocity, and the velocity and the number of returns seem to be the same thing; so that the same factor is introduced twice; and hence the square of the temperature, instead of the simple temperature, results.

In Prop. IX. and its cors. which depend upon the last, he asserts, in a somewhat positive manner, that MM. Dulong and Petit are mistaken in the result drawn from their experiments; but without entering upon any examination of their reasoning, and solely on the authority of his own theory. Thus even granting the validity of the proof above considered, he is assuming an hypothesis producing a result at variance with experiments (by his own confession), and in consequence rejecting the experiments.

The third cor. to Prop. IX. appears to contain an expression which stands much in need of elucidation. Mr. H. says, that the ratio of the temperature of freezing water to that of boiling is as 6 to 7 nearly. In a former paper on this subject, published in the *Annals* for July, 1816, he has warned his readers that he does not use the word "temperature" in its usual sense, though he by no means makes it clear in what sense he does use it. The only sense which the expression seems to me to admit in this place is, the heat which has been communicated to a body above that which can be conceived an absolutely cold state; and then the proposition must imply, that the gas has gone on expanding by successive communications of heat from a state in which it had no heat at all; because it is only established on the ground of that theorem which asserts temperature to be in a certain ratio to volume; and this is deduced only from experiments made within the limits at which heat expands the bodies employed; but both from the law laid down by Mr. H. and because heat is the cause which keeps atoms asunder, it follows, that when the temperature is 0, the volume must be 0 also; or, in other words, the gas must then not have existed. Thus by temperature we are to understand a certain degree of heat above that which thrown into a nonentity shall expand it into existence.

Mr. H.'s theory certainly affords a good explanation of the cause why all gases have a tendency to mix; but it appears to me that the explanation of their mixture on common principles does not necessarily involve the contradiction which he points out. If the gaseous state of a body be owing to the repulsion of its particles, and if we suppose the surfaces of two different gases to be in contact, then the particles which form the surface of each, will, by their repulsive force, endeavour to fly off from the masses to which they respectively belong; and they will fly off among the particles of the other gas, there being nothing to prevent them, till they reach the surface of the containing vessel; and this being the case with both gases, they must necessarily mix throughout the whole space which contains them.

Of the theorem given in p. 403, our author has not given us the least clue to a demonstration. In the *Annals* for July, 1816, it is given as a cor. to a more general theorem, which, I own, appears to me involved in an inconsistency. According to Prop. VIII. elasticity varies as the numeratom × square of temp. By this theorem it is as the square of temp. × the square of the numeratom, and inversely as the specific gravity.

The inference which he makes from hence respecting the composition of water appears to be directly at variance with the clear result of all experiments. Yet Mr. H. admits that either may be true, or neither, but maintains that it is beyond our power to demonstrate which is the case. The established doctrine upon this point is surely demonstrated to be the true one, if any ever was. He alludes to the subject in his paper published in 1816; and there says, that the common theory takes for granted that equal volumes of any two gases, *cæteris paribus*, contain an equal number of particles. But I beg to suggest whether this supposition is at all made. We find by experiment that the proportion of 2 hydrogen to 1 oxygen holds good whatever be the volumes we try, and thence we clearly and rightly infer, that the same must also be the case when the volumes are infinitely small, or atoms; but it is admitted that the atom of oxygen is of greater weight or density, and, therefore, contains more matter. Thus Mr. H.'s view of the subject may possibly not be inconsistent with the atomic theory. Though his definition of atoms is not easy to be conceived (see *Annals*, July, 1816), I would propose to his further consideration one difficulty attending it. Some atoms, he admits, may be composed of smaller particles; and, therefore, there can be no repulsions or collisions among these particles, yet there is between the "little individual bodies" formed by them, and the other atoms of the gas; and as they must all be of the same kind, and endued with the same properties, there is a difficulty in conceiving how some of them come to unite, and others to repel one another, which needs some explanation.

The theorem which he gives for the temperature of a mixture in p. 403 is left without explanation; and as the results derived from it agree nearly with those from other sources, there is of course very little evidence gained in favour of either theory. But the theorem in question rests upon the existence of a point of absolute cold; and, therefore, we must hesitate in admitting it till we have made up our minds on that much controverted point. Mr. H. has not proved that any such point exists; and till that is done, it is premature to think of finding an expression to represent it.

Mr. H. also endeavours to explain those phenomena which are usually ascribed to latent heat; a theory which if any theory was ever established by the most direct and decisive experiments, and by the most clear and forcible reasoning, must be eminently considered so; yet he rejects it, and attempts to explain the phenomena on his own principles: his reasoning, however, appears to me inconclusive.

His principal step is, that two particles in motion uniting, the motion of this new particle must be compounded of the motions of the others which compose it; and, therefore, will be greater than the motion of either of the constituent parts before aggregation.

But it does not follow that two particles, each moving with a certain velocity, should, when they unite, necessarily move with a greater velocity. If they were moving in opposite directions, and were suddenly united, their motion would cease altogether. If they were moving in directions inclined towards each other at any angle, they would each lose a part of their motion, and the resulting motion would be that arising from the composition of the remaining motions of each. But this is taking for granted that they unite in such a manner as will not affect their motions by the act of union. This, however, cannot be admitted.

An union cannot take place without some force acting upon one or both of the particles. Thus each particle would be urged by two motions; that with which it was before moving, and that with which it approaches the other particle; and, therefore, its motion in the former direction must be diminished.

It is also what, I think, Newton would call "durior hypothesis," to conceive that the particles of a solid should move among themselves with greater velocity than those of a fluid.

Thus I have, I hope without offence, stated a few objections and difficulties which have occurred to me in reading Mr. Herapath's papers. Should these remarks be thought worth attention, I may, perhaps, trouble you with a few more on the other parts of his inquiries; and with such remarks, if communicated with candour, Mr. H. himself cannot be displeased, as he has professed it his wish to excite inquiry and examination into the validity of his system. I am, &c. X.

ARTICLE XIV.

ANALYSES OF BOOKS.

Philosophical Transactions of the Royal Society of London for the Year 1821. *Part I.*

This part contains the following papers :

I. *On the Black Rete Mucosum of the Negro, being a Defence against the scorching Effect of the Sun's Rays.* By Sir Everard Home, Bart. FRS.

The author of this paper remarks, that to ascertain the use of the black colour of the rete mucosum in the negro has occupied the attention of many physiologists. Sir Everard relates various experiments which he performed to elucidate this curious subject. From these, I shall select two which indicate the most remarkable differences in the power of the sun's rays upon white and black skins.

" I exposed," says the author, " the backs of my two hands to the sun's rays, with a thermometer upon each ; the one hand was uncovered, the other had a covering of black cloth, under which the ball of the thermometer was placed. After ten minutes, the degree of heat of each thermometer was marked, and the appearance on the skin examined. This was repeated at three different times. The

1st time, the therm. under the cloth,	91° ;	the other,	85°
2d ————————————	94	————	91
3d ————————————	106	————	98

" In every one of these trials, the skin was scorched that was uncovered ; the other had not suffered in the slightest degree ; there was no appearance of perspiration on either hand."

" The back of a negro's hand was exposed to the sun with a thermometer upon it, which stood at 100° ; at the end of 10 minutes, the skin had not suffered in the least."

Sir E. Home concludes from these and other experiments, " that it is evident that the power of the sun's rays to scorch the skin of animals is destroyed when applied to a black surface, although the absolute heat, in consequence of the absorption of the rays, is greater." This fact, the author informs us, was explained by Sir H. Davy, by observing that " the radiant heat in the sun's rays was absorbed by the black surface, and converted into sensible heat."

II. *On the Magnetic Phenomena produced by Electricity.* In a Letter from Sir H. Davy, Bart. FRS. to W. H. Wollaston, MD. PRS.

This paper is printed in the present volume of the *Annals.*

III. *A Communication of a singular Fact in Natural History.* By the Right Hon. the Earl of Morton, FRS. In a Letter addressed to the President.

Some account of this communication has been already given in the *Annals.*

IV. *Particulars of a Fact, nearly similar to that related by Lord Morton.* Communicated to the President, in a Letter from Daniel Giles, Esq.

V. *The Croonian Lecture.—Microscopical Observations on the following Subjects: On the Brain and Nerves; showing that the Materials of which they are composed exist in the Blood; On the Discovery of Valves in the Branches of the Vas Breve, lying between the Villous and Muscular Coats of the Stomach; On the Structure of the Spleen.* By Sir Everard Home, Bart. VPRS.

This paper cannot of course be rendered intelligible without the plates which accompany it.

VI. *On Two New Compounds of Chlorine and Carbon, and on a new Compound of Iodine, Carbon, and Hydrogen.* By Mr. Faraday, Chemical Assistant at the Royal Institution. Communicated by W. T. Brande, Esq. Sec. RS. and Prof. of Chemistry at the Royal Institution.

This very interesting paper has been given in the present volume of the *Annals.*

VII. *An Account of the Comparison of various British Standards of linear Measure.* By Capt. Henry Kater, FRS. &c.

The commissioners appointed to consider the subject of weights and measures recommended in the First Report, " for the legal determination of the standard yard, that which was employed by General Roy in the measurement of a base on Hounslow Heath," as a foundation for the trigonometrical operations that have been carried on by the ordnance throughout the country. In consequence of this determination, says Capt. Kater, it became necessary to examine the standard to which the Report alludes, with the intention of subsequently deriving from it a scale of feet and inches. The object of this paper is to detail the experiments for this purpose, and they appear to have been conducted with the usual precaution and ability of their author. Comparative measures of various standards are given in the form of tables; and the following one contains the results deduced by comparing each standard in succession with that used by Col. Lambton in the Survey of India, an account of whose operations may be found in the Phil. Trans. for 1818.

Excess of the following standards above Col. Lambton's standard.	On 36 inches.
Sir G. Shuckburgh's standard	+ ·000642
Bird's standard of 1760.	+ ·000659
General Roy's scale	+ ·001537
Royal Society's standard	+ ·002007
Ramsden's bar (used in the Trigonometrical Survey of Great Britain)	+ ·003147

The standard used in the Trigonometrical Survey, being thus unexpectedly found, Capt. Kater observes, to differ so considerably from every other standard of authority, the Commissioners of Weights and Measures proposed in their second report, that Bird's parliamentary standard of 1760, should be considered as the foundation of all legal weights and measures.

It is remarked by Capt. Kater, "that the standard thus selected differs so little, if at all, from that of Sir G. Shuckburgh, that they may, for every purpose, be considered as perfectly identical; and this agreement is particularly convenient, because the length of the metre having been determined by comparisons with Sir G. Shuckburgh's scale, and a fac simile of it made by Mr. Troughton, for Prof. Pictet, all measures on the Continent are converted into English measures by reference to Sir George Shuckburgh's standard."

VIII. *An Account of the Urinary Organs and Urine of two Species of the Genus Rana.* By John Davy, MD. FRS.

An account of the contents of this paper has been given in the *Annals*.

IX. *An Account of a Micrometer made of Rock Crystal.* By G. Dollond, FRS.

This paper cannot be rendered completely intelligible without reference to the drawing which accompanies it. According to Mr. Dollond the advantages to be derived from his improvement are the following: that in making a sphere or lens from a piece of rock crystal, and adapting it to a telescope in the place of the usual eye glass; and from its natural double refracting property, rendering it useful as a micrometer.

The advantages of thus applying the crystal are, in the first place, the very great saving of the time required to find the proper angle for cutting the crystal, also of cutting the crystals to their proper angles, and working their surfaces with sufficient accuracy to render them useful as micrometers in the manner that is recommended by M. Arago, Dr. Wollaston, and others.

Upon the plan which is now submitted, it is only necessary to select a piece of perfect crystal; and without any knowledge of the angle that will give the greatest double refraction, to form the sphere of a proper diameter for the focal length required.

The second advantage is derived from being able to take the angle on each side zero, without reversing the eye tube; also of taking intermediate angles between zero and the greatest separation of the images, without exchanging any part of the eye tube, it being only required to move the axis in which the sphere is placed.

Thirdly, it possesses the property of an eye tube or lens that is not intended for micrometrical measurements; for when the axis of the crystal is parallel to the axis of the object glass of the telescope; only one image will be formed, and that will be as distinctly formed as with any lens that does not possess the double refracting property.

The eye tube is so constructed that the plane through which the two images move can be placed parallel to the line in the object which is to be measured; and if this motion is furnished with a divided circle, it will correctly answer the purpose of a position micrometer.

The value of the scale is found from the known diameter of any distant object, and will vary in proportion to the magnifying powers of the eye tube; its value increasing in proportion to the increase of those magnifying powers.

After referring to the plate which is needful to render the nature of the instrument perfectly intelligible, Mr. Dollond concludes with observing, "When I constructed this micrometer, it was my intention to have applied it to the measurement of the angles that are subtended by the apparent diameters of the fixed stars, as seen in achromatic refracting telescopes, for the purpose of determining their relative magnitudes: also of measuring the distances of those double stars that would come within the range of the micrometer; but from being compelled to attend to business of more immediate consequence, I am not able to accompany this description with any measurements that are sufficiently important to be interesting; although I am fully convinced from the trials I have made that the micrometer is quite equal to the purposes for which it was intended."

X. *The Bakerian Lecture.—On the best Kind of Steel and Form for a Compass Needle.* By Capt. Henry Kater, FRS.

The experiments which form the subject of the present paper were undertaken by Capt. Kater, in consequence of its having appeared, on the return of the first expedition which sailed for the discovery of a North-West Passage, that from the near approach to the magnetic pole, and the consequent diminution of the directive force, the compasses on board had become nearly useless. Capt. Kater was, therefore, anxious that the next expedition should be furnished with instruments, combining as much power and sensibility as possible.

After numerous experiments as to the material, form, polish, and other circumstances connected with the power and sensibility of the needle, which it would be scarcely possible to abridge, Capt. Kater arrived at the following conclusions, which are given in his own words.

"That the best material for compass needles is *clock spring*; but care must be taken in forming the needle to expose it as seldom as possible to heat; otherwise its capability of receiving magnetism will be much diminished.

"That the best form for a compass needle is the *pierced rhombus*, in the proportion of about five inches in length to two inches in width, this form being susceptible of the greatest directive force.

"That the best mode of tempering a compass needle is, first, to harden it at a red heat, and then to soften it from the middle to about an inch from each extremity by exposing it to a heat sufficient to cause the blue colour which arises again to disappear.

"That in the same plate of steel, of the size of a few square inches only, portions are found varying considerably in their capability of receiving magnetism, though not apparently differing in any other respect.

"That polishing the needle has no effects on its magnetism.

"That the best mode of communicating magnetism to a needle appears to be by placing it in the magnetic meridian, joining the opposite poles of a pair of bar magnets (the magnets being in the same line), and laying the magnets so joined flat upon the needle with their poles upon its centre; then having elevated the distant extremities of the magnets, so that they may form an angle of about two or three degrees with the needle, they are to be drawn from the centre of the needle to the extremities carefully preserving the same distance from the needle. The operation is to be repeated 10 or 12 times on each surface.

"That in needles from five to eight inches in length, their weights being equal, the directive forces are nearly as the lengths.

"That the directive force does not depend upon extent of surface, but in needles of nearly the same length and form is as the mass.

"That the deviation of a compass needle occasioned by the attraction of soft iron, depends, as Mr. Barlow has advanced, on extent of surface, and is wholly independent of the mass, except a certain thickness of the iron, amounting to about two-tenths of an inch, which is requisite for the complete developement of its attractive energy.

XI. *Notice respecting a Volcanic Appearance in the Moon.* In a Letter addressed to the President, by Capt. Henry Kater, FRS.

XII. *A further Account of Fossil Bones discovered in Caverns inclosed in the Limestone Rocks at Plymouth.* By Joseph Whidbey, Esq. In a Letter addressed to Sir Everard Home, Bart. VPRS.

XIII. *On the Aëriform Compounds of Charcoal and Hydrogen, with an Account of some additional Experiments on the Gases from Oil and Coal.* By William Henry, MD. FRS.

This very excellent paper is given at length in the present number of the *Annals*.

Article XV.

Proceedings of Philosophical Societies.

GEOLOGICAL SOCIETY.

June 1.—A paper, on the Formation of Basalt, by Mr. Lillingstone, was read.

Mr. Lillingstone conceives that basaltic formations possess a degree of gravitation towards their own centres, which does not belong to other strata, as may be proved by an examination of the joints in the Giant's Causeway, and of the decomposition which is taking place in the basaltic mass opposite Queensferry, where an original spherical centre is demonstrated by the decay of the surrounding mass. This spherical attraction is also exhibited to a certain extent in the red marl strata (in which the spheres exist) on the shore of the Clyde, nearly opposite Greenock, and in many other places. It is to the operation of this principle that Mr. L. is disposed to refer the formations in question.

The reading of Mr. Strangway's paper on the Geology of Russia was concluded.

The central mining district, which includes parts of the governments of Nishny, Novgorod, Vladimir, Tamblof, Rezan, Toula, and Calonga, extending from a little above Mourom, on the Oca, to near the town of Calonga, is, in general, a poor sandy country, probably belonging to the red rock formation, though its connexions are not very distinct. At the depth of 60 feet below the surface of the soil is found a series of beds of ironstone, of variable quality. The lightest coloured ore yields the most iron. In general, it is manufactured where it is raised.

Across the middle of Russia, or from the reach of Samara on the Volga, to the country between Smolensk and Moscow, a tract of limestone extends, generally of a very pure white, and completely filled with broken encrinites, large tetrabratulites, caryophyllites, pectinites, and the exuviæ of other marine animals. This white limestone occurs also in great quantity in that part of the country above Mourom where the government of Nishny-Novgorod and Tamblof join those of Vladimir and Rezan. Further outward it appears in the southern portion of the government of Simbersk; and on the banks of the Volga is seen for a considerable distance both above and below the town of Cinghyley, forming the lofty ridge which diverts the course of the river between Stavropol and Syzran, called the Markvashy and Shigoulifky hills. At Sernoi-Gorodsk, it contains sulphur mines, which are no longer worked.

The Oural mountains, which extend form the Icy Sea to the Steppe, north of the Caspian Sea, form the natural boundary between Asiatic and European Russia. An extensive district of red marl, salt, and gypsum, stretches down the course of the Kama, and is probably connected on the south with the salt district of the Volga. On both sides of this salt country, and skirting the south and west sides of the Oural mountains, is a vast tract of a dull red or a green sand, commonly called copper-sand, and worked for copper. It extends through great part of the governments of Viatka, Perin, and Orifa.

Of the *Steppe* district, the primitive tract may be described as stretching in a direction ESE from the upper part of the river Bug to the Berda, and terminating within a short distance of the Black Sea. It is a coarse grained granite, containing garnets, but sometimes passing into trap or syenite. In Volhynia, near the borders of Gallicia, it affords a fine white earthy felspar. A series of calcareous rocks accompany the southern border of the primitive Steppe, in which, towards the frontier of Gallicia, and near Tomaspol, some large grained oolites appear. A shelly limestone resembling that of Purbeck and Portland also occupies a large tract in the vicinity of the last named situation, between the rivers Bug and Dniester.

The greater part of the interior of the Crimea appears to consist of similar strata, the only new formation being the bituminous peninsula of Korch, and at the other end of the Caucasian chain in the promontory of Bacou. The bituminous formation reappears in the Isles of Naphtha, on the eastern shore of the Caspian, and it is said also in Georgia.

The *Salt Steppe* lies at an extremely low and generally uniform level, extending between the Black Sea and the Caspian. The lakes and pools which it contains are mostly salt; the rock under the superficial sand, and sometimes left bare, is a hard clay. Its origin is usually referred to a change of level in the waters of the Black Sea, which, having burst a passage through the Straits of Constantinople, left the shallow tract between them and the Caspian perfectly dry.

The Caucasus is a primitive chain, containing, in many places, columnar trap. On its northern border, the older secondary rocks are a continuation of those which form the highest mountains on the south coast of the Crimea, and which are principally composed of slate, with a conglomerate and older limestone.

A letter was read from Mr. Parkes, concerning the Black Oxide of Manganese found in Warwickshire.

The specimens which accompanied this paper appear to be of a different character from those obtained from Cornwall, Devonshire, and Scotland; and contain more oxygen. They were found at a place called Hartshill, near the towns of Atherstone and Nuneaton, in the county of Warwick; occurring in detached pieces of from 1 to 50 or 60 pounds in weight, at the depth of from one foot to six or eight feet, below the surface of the soil, which is chiefly clay.

A paper, on the Bagshot Sand, by Henry Warburton, Esq. VPGS. was read.

The sand of Bagshot Heath occupies a district of about 25 miles in length, extending from east to west along the axis of the chalk of the London basin; commencing at Esher, in the county of Surrey, and terminating at Bromshill Common, on the confines of Berkshire and Hampshire; its greatest breadth in a line drawn from Hungry Hill, near Farnham, to Oakingham is about 12 miles. The course of its southern boundary is marked by those sandy elevations, which, beginning at Esher, extend to Painshill, Breach Hill, and Ockham Hill, near Ripley. At Tukebury Hill, and Beacon Hill Camp, immediately south of Farnham, it attains its greatest elevation; and approaches within less than a mile of the ridge of chalk which forms the southern limit of the London basin. From Beacon Hill Camp, its western boundary may be traced over a low moorish country to Hertford Bridge, where it again acquires considerable thickness, forming a regular escarpment to Broomshill Common. There its northern boundary commences, passing to the hill above Egham, and forming a line of sandy hillocks, parallel to the valley of the Thames, and at no great distance from the river, by St. Ann's Hill and Oatlands, to the eastern extremity at Esher.

At Egham Hill, the sand is observed resting immediately on the London clay, and there is every reason to believe that it retains the same position throughout the district which it occupies. Those parts which are nearest to the surface are generally more or less mixed with angular chalk flints, and other diluvian debris; such as are found upon almost every part of the London basin. The highest of the undisturbed beds consist of a meagre and somewhat ochreous sand, without any angular masses of flint; and, at a lower level, beds of foliated green clay, alternating with beds of green sand, occur, as may be observed to the north of Chobham Park, on the road from Chertsey to Bagshot. Below these beds are found alternating strata of white, sulphur yellow, and pinkish foliated marls, containing abundant grains of green sand regularly stratified, and inclosing fossil shell, which have not hitherto been found in England in any other bed above the chalk. These lowest beds, which may be seen in descending the acclivity to the south of Chobham Park, appear to be about 40 feet thick. Of the shells found in them, the most abundant is the cast of a crassatella, agreeing with one found in the Paris basin at Meudon in the *calcaire grossière à matiere verte.*

At St. Ann's Hill, the beds which lie nearest to the London clay consist of masses of rolled chalk flints resembling those of the pebble bed in the plastic clay, intermixed with green sand, green foliated marl, and stony concretions of the sandstone, which are so generally disposed in masses over the surface of

Bagshot Heath. In digging a watercourse for the purpose of draining the newly inclosed lands of Windsor Forest on the road that leads from Hollyport to Binfield, where the sand with its concretions rests upon variegated plastic clay, numerous turbinated and other fossil shells have been found; also a shark's tooth, pyritous wood, and the seed vessel apparently of a marine plant; but the cerithia, which are found in the similar beds in Sussex and near London, are wanting.

Article XVI.

SCIENTIFIC INTELLIGENCE, AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Gas from Cocoa Nut Oil.*

A large quantity of this oil has been lately imported into this country. It has been used, but, I believe, with considerable difficulty, in the manufacture of soap. It has rather a pleasant smell, and is about the consistence of butter; but what is singular is, that the soap made of it leaves a peculiar and extremely unpleasant smell. Messrs. Taylors and Martineau inform me that they have lately used it in their apparatus for the production of gas, which gives an extremely brilliant and white light by combustion. According to their report, it may be economically employed for this purpose; and, on account of its solid form and pleasant smell, it is much preferable to the oil commonly used, especially in private houses.—*Ed.*

II. *On the Crystallization of Sugar under a particular Circumstance.*

By M. Henry Braconnot.

Crystallization sometimes takes place under circumstances so remarkable that I think we should not neglect the slightest facts, connected with the theory of this wonderful property of matter. The following seems in opposition to the generally admitted fundamental principles, according to which, bodies can only crystallize when their molecules have free motion in a fluid, so that they can approach towards each other.

Every one knows that fresh prepared barley sugar is perfectly transparent, has a vitreous very glossy fracture, and consequently its internal, homogeneous parts present no appearance of crystallization; but after some days, its surface begins to tarnish, and becomes covered with a crystalline pellicle, which goes on increasing till the barley sugar *drop*[*] is entirely crystallized, when it has lost part of its transparency, and is converted into rounded groupes of radiating needle crystals, generally separated by vacant spaces, or gaps, which did not previously exist; whence it follows, that the molecules have formed, and approached each other, in the very bosom, and at the expense of a hard compact

[*] Tablette.

substance—a circumstance which seemed to oppose an insurmountable obstacle to their regular arrangement. Barley sugar, so crystallized, is much more brittle than before; its fracture presents a multitude of small, fibrous, diverging, acicular crystals, collected in numerous *bundles*,* terminated by interstices, provided this sort of crystalline arrangement has taken place slowly; that is, below the mean temperature; when held for some time in the mouth, instead of remaining glossy and polished, it becomes full of *hollows* and asperities: with some care, we may separate the needle crystals, which appear, when viewed by the microscope, to be flattened tetrahedral prisms. According to the known conditions of crystallization, it was to be presumed, that that of barley sugar could only proceed by its gradually attracting moisture from the air; but having left some for a month in a close stopped bottle, containing chloride of calcium, the sugar lost about 1-200th of its weight, and crystallized quite as well as in the free air. In oil of turpentine, the same result was obtained.

The confectioners are aware of, and fear the effects of, this singular crystallization of barley sugar, which they call its *dying*, seeing nothing in this tendency to perfection, but an insensible degradation. They would wish to find the means of preventing it, but it appears that nothing can hinder it from taking place.—(Ann. de Chim. xvi. 427.)

III. *New Mineral Substance.*

Mr. J. Deuchar found, a few weeks ago, a new mineral substance imbedded in striped limestone. It melts at the candle, and burns on a wick, or paper. In the cold, it is insoluble in alcohol, potash, or oil of turpentine; nor is it acted upon in the cold, after five days' exposure to sulphuric, muriatic, or nitric acids. He is now engaged with its analysis.

IV. *On Compounds of Sulphur with Cyanogen, &c.*

M. Berzelius, in pursuance of his Researches on the Compounds of Cyanogen (p. 208), has lately examined the sulphuretted compounds of cyanogen, and added much to our knowledge of them. He concludes that the substance, as prepared by M. Grotthus or M. Vogel (i. e. by fusing sulphur with ferroprussiate of potash, dissolving, filtering, and drying), is a sulphocyanuret of potassium; and though he has not been able to separate the sulphocyanogen or sulphuret of cyanogen from the base, so as to have it in a separate state, yet he deduces its composition from experiments, as being 1 atom cyanogen, and 2 atoms of sulphur, or

Carbon.......	20·63	....	2 atoms	....	150·66
Azote.........	24·23	....	1 atom	....	177·26
Sulphur......	55·09	....	2 atoms	....	402·32
	100·00				730·24

The sulphocyanuret of potassium is composed of

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* Faisceaux.

Potassium....	40·15	.... 1 atom		979·83
Azote.......	14·53	.... 2 atoms	354·52	1460·48
Carbon......	12·35	.... 4 atoms	301·32	
Sulphur.....	32·97	.... 4 atoms	804·64	
	100·00		2440·31	

The sulphuretted hydrocyanic acid is composed of

Hydrogen....	1·68	.... 2 atoms	12·44
Nitrogen......	23·85	.... 1 atom	177·26
Carbon.......	20·30	.... 2 atoms	150·66
Sulphur......	54·17	.... 2 atoms	402·32
	100·00		772·68

In considering the nature of these substances, M. Berzelius seems inclined to admit that view of them and their nature which is analogous to the chlorine theory, as also this theory itself, and goes a considerable way towards answering some of those objections which have been raised at different times to it.

On substituting selenium for sulphur in these and analogous experiments, results which might have been expected from the analogy of the two bodies, took place. On heating it with the ferroprussiate of potash, a seleniocyanuret of potash was formed perfectly analogous to the sulphocyanuret.—(Ann. de Chim. xvi. 23.)

ARTICLE XVII.

NEW SCIENTIFIC BOOKS

PREPARING FOR PUBLICATION.

A Practical Treatise on Diseases of the Liver, and on some of the Affections usually denominated Bilious; comprising an impartial Estimate of the Merits of the Nitromuriatic Acid Bath. By George Darling, MD. Member of the Royal College of Physicians of London.

An Introduction to Entomology; or Elements of the Natural History of Insects. Vol. III. By William Kirby, MA. FRS. and LS. and William Spence, Esq. FLS. Illustrated by coloured Plates.

JUST PUBLISHED.

The Elements of Astronomy, designed for the Use of Schools and Junior Students. By S. Treeby, with Plates. 18mo. bound. 3*s*. 6*d*.

Medico-Chirurgical Transactions, published by the Medical and Chirurgical Society of London. Vol. XII. Part II. 8vo. 9*s*.

The Principles of Forensic Medicine, systematically arranged and applied to British Practise. By J. G. Smith, MD. 8vo. 14*s*.

A Treatise on Indigestion and its Consequences, called Nervous and Bilious Complaints. 8vo. 9*s*.

Vegetable Materia Medica of the United States, or Medical Botany: containing a Botanical, General, and Medical History of Medicinal Plants, indigenous to the United States. Illustrated by coloured Engravings, made after Original Drawings from Nature. By William P. C. Barton, MD. 2 vols. 4to. 6*l.* 6*s.*

Barton's Flora of North America. Ten Numbers. 6*s.* each.

An Illustration of the Genus Cinchona, comprising Descriptions of all the Officinal Peruvian Barks, &c. By A. B. Lambert, Esq. FRS. 4to. 1*l.* 10*s.* 6*d.*

Article XVIII.

NEW PATENTS.

Sir William Congreve of Cecil-street, Strand, Middlesex, Bart. and James Nisbet Colquhoun, of Woolwich, Kent, Lieutenant in the Royal Artillery; for certain improvements in the art of killing and capturing whales, and other animals to which such means are applicable.—June 7, 1821.

John Vallance, of Brighton, Sussex, brewer; for improvements on a patent granted to him on the 20th of June, 1820, for a method and apparatus for freeing rooms and buildings (whether public or private) from the distressing heat sometimes experienced in them, and of keeping them constantly cool, or of a pleasant temperature, whether they are crowded to excess or empty; and also whether the weather be hot or cold; and the said John Vallance hath invented or discovered improvements relative thereto, and in some cases with, and in some cases without, a gas or gases extended, or additional applications of the principles, or of some or one of the principles (either of construction or operation) thereof, as applicable to purposes other than what he first contemplated.—June 19.

James Simpson, of the Strand, Middlesex, surgical instrument maker; for an improvement in the manufacture of snuffers.—July 3.

William Church, of Threadneedle-street, London, gentleman, for an improved apparatus for printing.—July 3.

William Coles, of New-street-square, London, mechanic, for braces, or instruments, for the relief of hernia or ruptures.—July 5.

Robert Dickinson, of Great Queen-street, Middlesex, Esq. for certain improvements in the construction of vessels, or crafts, of every description, whereby such vessels, or crafts, may be rendered more durable than those heretofore constructed for the purposes of navigation.—July 14.

Charles Newman, of Brighton, Sussex, coachmaster, for an improvement in the construction of the body and carriage of a stage or other coach, by placing a certain proportion of the outside passengers in the centre of the carriage, and a proportion of the luggage under the same, producing thereby safety to the coach, and convenience to the passengers.—July 17.

Samuel Cooper, engineer, and William Miller, gentleman, both of Margate, Kent, for certain improvements in printing machines.—July 17.

ARTICLE XIX.

METEOROLOGICAL TABLE.

1821.	Wind.	Barometer. Max.	Barometer. Min.	Thermometer. Max.	Thermometer. Min.	Evap.	Rain.	Hygr. at 9 a.m.	
7th Mon.									
July 1	W	29·83	29·60	74	49	—	86	72	
2	E	29·83	29·81	65	48	—	28	93	
3	N E	30·09	29·81	62	36	—		73	
4	N	30·20	30·09	70	46	—		65	
5	N W	30·20	30·03	67	49	39		62	
6	N W	30·03	29·88	65	49	—	02	70	
7	N	30·06	29·88	63	40	—	07	75	☽
8	N	30·12	30·06	65	44	—		74	
9	N W	30·14	30·09	68	53	—		66	
10	N W	30·16	30·14	70	43	48		67	
11	N W	30·14	30·10	71	42	—		67	
12	S E	30·10	30·03	75	38	—		67	
13	S	30·03	29·90	73	49	—		66	
14	N W	29·90	29·77	72	50	—	09	79	
15	N W	30·02	29·77	64	46	43	48	85	●
16	N W	30·26	30·02	73	45	—		76	
17	Var.	30·32	30·26	75	43	—		71	
18	S E	30·32	30·13	75	47	—		64	
19	E	30·13	29·89	78	52	50		79	
20	W	29·89	29·82	74	57	—		66	
21	W	29·82	29·64	75	54	—	21	71	
22	S W	29·67	29·64	73	50	—		72	☾
23	W	29·78	29·67	68	53	55	30	92	
24	S W	29·68	29·67	70	57	—	21	75	
25	N W	29·92	29·68	72	53	—	04	75	
26	S W	29·95	29·92	70	54	—		73	
27	W	26·97	29·95	71	47	57		75	
28	S E	30·04	29·97	70	47	—		74	
29	S W	30·04	29·95	69	53	—	08	70	○
30	S	29·95	29·94	70	58	—	18		
31	W	29·94	29·94	74	62	35			
		30·32	29·60	78	36	3·27	2·82	93—62	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

REMARKS.

Seventh Month.—1. Rainy. 2, 3. Cloudy. 4. Fine: *Cirrus.* 5. Cloudy and fine: *Cirrus.* 6. Cloudy. 7. Cloudy: showers. 8. Cloudy. 9—12. Fine. 13, 14. Cloudy. 15. Rainy. 16, 17. Cloudy. 18. Fine. 19. Fine: some thunder in the evening, and a brilliant meteor. 20—22. Fine. 23. Showery: some hail at four, p.m. 24, 25. Showery. 26. Cloudy. 27, 28. Fine. 29. Cloudy. 30. Showery. 31. Fine.

RESULTS.

Winds: N, 3; NE, 1; E, 2; SE, 3; S, 2; SW, 4; W, 6; NW, 9; Var. 1.

Barometer: Mean height	
For the month	29·960 inches.
For the lunar period, ending the 22d	30·023
For 13 days, ending the 5th (moon north)	30·048
For 14 days, ending the 19th (moon south)	30·058
Thermometer: Mean height	
For the month	59·596°
For the lunar period	58·483
For 31 days, the sun in Cancer	57·983
Evaporation	3·27 in.
Rain	2·82
Mean of hygrometer	72°

Laboratory, Stratford, Eighth Month 16, 1821. **R. HOWARD.**

ANNALS

OF

PHILOSOPHY.

OCTOBER, 1821.

ARTICLE I.

On Floetz Formations.[*] By Thomas Weaver, Esq. MRIA. MRDS. MWS. MGS.

THE following remarks were called forth in part by a perusal of M. D'Aubuisson's Traité de Géognosie,[†] in the autumn of 1820. They were intended in part, also, as an appendix to a paper written by me on some parts of Gloucestershire and Somersetshire, and which was read some time since before the Geological Society; but observing that other geologists, both foreign and British, are partly disposed to entertain the same views as M. D'Aubuisson, I think it right not to delay in giving them publicity.[‡]

In my memoir on the East of Ireland (Geol. Trans. vol. v.) I have spoken of the old red sandstone, limestone, and coal formation, of that country, as belonging to the *first floetz series;* and I have adverted to similar formations existing in England and Wales. But M. D'Aubuisson remarks (vol. ii. p. 253, 254, and 313, 314), that the sandstone in question is, perhaps, a greywacke, and the limestone, the transition limestone of Werner.

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[*] For the term *floetz*, some French, Italian, and English, writers substitute those of *secondary* and *tertiary;* but the only sense in which I use the word secondary is the Wernerian one; comprehending both the transition and floetz formations, in contradistinction to the primary.

[†] Published in 1819, in two volumes, 8vo.

[‡] I think it also right to state, that this article was digested before the appearance of Prof. Buckland's Comparative View of the Formations in England and the Alps: a view highly valuable and instructive, and affording ample evidence of the indefatigable research and discriminating powers of the author.—(See *Annals of Philosophy* for June, 1821.)

Hence I find it necessary to give a fuller exposition of my views on this subject, in which I propose to show that a perfect identity subsists between those formations in the British isles and on the Continent; and that in fact they all three belong to the first floetz period.*

I enter upon this task the more willingly, as there are, perhaps, few branches of geological inquiry more instructive than that which relates to the occurrence of the same class or group of formations in different parts of the world; and in a practical point of view, this investigation is of great importance to the subminer. The more attentively we consider this part of our subject, the more readily we shall probably assent to the proposition; namely, that a certain general order or progression prevails in the structure of the earth, from the oldest to the newest formations; that the *detail* of this order, however, is not constantly the same, but varies in different countries, and sometimes even in the same tract of country.

On the Floetz, First Series, as comprehending the Formations of

In the British Isles.	*In Germany.*
1. Old red sandstone.	1. The *rothe todtliegende* of Lehman, Werner, Karsten, Von Buch, Heim, Freiesleben, &c. The old, or *first floetz sandstone formation* of Werner.
2. Mountain, or carboniferous † limestone.	2. The limestone subordinate to the rothe todtliegende of ditto.
3. Coal formation.	3. The coal subordinate to the rothe todtliegende, and The coal formation reposing upon it. *Eigentliche Steinkohlen formation* of ditto.

In the Netherlands.

1. Poudingues et grès rouges.	Of French geologists.
2. Calcaire intermédiare.	
3. Grès houiller.	

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* That is, according to their true position in the geological series, and always considering the old red sandstone as the *first* member of the floetz class, which rank it has always held, even from the time of Lehman. To denominate that formation a greywacke is to have little respect to mineralogical character, or to general relative position, by both of which it is sufficiently distinguished, although, in point of affinity and age, it stand next to the transition conglomerate, and sandstone. So forced a construction seems to have originated in a loose expression of Werner, respecting the Derbyshire limestone, which was said to be transition at a time when its relations had not been fully

In the British Isles.

In no part of the world, perhaps, are the mutual relations of these three formations more fully displayed, in their distinct order of progression, and in their various states of association, than in England, Scotland, and Ireland. A general view of those relations will tend to show that these formations must be considered as collectively constituting one group of the same era; and the whole series, whether in complete or incomplete progression, are always found reposing on transition, or on primary tracts, or on both of these conjointly.

1. *Ireland.*—In those parts of Ireland, of which I have given an account (Geol. Trans. vol. v), the formations of old red sandstone, limestone, and coal, are separate and distinct, the lines of division between them being broadly marked. This example may be considered as affording the perfect type of the series in their distinct order of succession. It may, however, be remarked, that the old sandstone being there of unequal distribution, the limestone formation is, in certain parts of its extent, in direct contact with transition, and in others with primary tracts.

This latter fact may be observed also in the north-west of England, where the carboniferous limestone nearly encircles the transition and primary tracts of Cumberland, Westmoreland, and Lancashire, the old red sandstone intervening in few instances. In other quarters again, the carboniferous limestone is altogether wanting, the coal formation being directly and conjointly in contact with old red sandstone and with transition rocks; or the

explored. The term has, in consequence, been misapplied in other tracts; and hence also it has been supposed, that the old red sandstone, which precedes it in the order of formation, must be a greywacke, or transition. But the real transition limestone of Werner is found more particularly in association with clayslate, and with the true greywacke of that naturalist, e. g. in the Hartz.‡ On the other hand, however, if the limestone in question (the English mountain limestone) be not the transition limestone of Werner, neither is it his first floetz limestone. It so happened, that in the tracts which came under the immediate consideration of the Freyberg Professor, in the north of Germany, the limestone, which is there associated with the old red sandstone, is found only in inconsiderable subordinate beds, incidentally disposed in that formation; and hence, if not entirely overlooked, it was not raised by him to the rank of a distinct formation; and hence also the application by him of the term first floetz limestone to that limestone in Germany, which, in respect to age, appears to correspond with what has been designated in England by the name of the magnesian limestone formation.§ Even the greatest men are subject to error, when information proves defective; but in the researches of the naturalist, the authority of names, however high, ought never to outweigh the cogency of facts.

† This term, which has been introduced by Prof. Buckland and the Rev. W. Conybeare, appears very appropriate, as distinctive of this associate of the great coal formation.

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‡ Or, as included in group No. 23 of Mr. Greenough's Geological Map of England and Wales; a truly spirited work, by the execution of which, a most important service has been rendered to geology.

§ Included in group No. 17 of Mr. Greenough's Geological Map. Wherever, in the following pages I may have occasion to refer to that map, I shall in general mention the number of the group, with the initials G. M. only.

old red sandstone being also wanting, the coal field reposes simply on a transition tract: of which examples may be observed in the coal fields of Shropshire.—(See Mr. Greenough's Geological Map of England and Wales.)

But, in general, in Great Britain, the three formations are found in a more or less intimate state of affiliation, to illustrate which the following examples may be adduced, selecting only such as are characteristic of their various modes of connexion.

2. *England.*—That part of the old red sandstone of *Herefordshire* and *Gloucestershire* (Group, No. 22 of G. M), which supports the forest of Dean tract, contains, incidentally, thin beds of limestone, and, also, thin seams of imperfect coal. It is succeeded by a belt of limestone, including beds of slate-clay (Group, No. 21 of G. M.), and supporting sandstone and sandstone conglomerate (Group, No. 19 of G. M.), which encloses the distinct coal basin (Group, No. 18 of G. M.) of the forest of Dean.

3. In the *Environs of Tortworth*, Gloucestershire, on the borders of the Cromhall coal basin, the old red sandstone alternates with the limestone, while the coal formation is distinct. Such appears likewise to be the structure of the western side of the coal basin in Anglesea.* In both these cases, also, the immediate foundation of the coal field is composed of the sandstone and sandstone conglomerate, commonly called *millstone grit*, which, in Ireland, is wanting in this position, the coal formation there reposing directly on the limestone.

4. In the *Durham* and *Northumberland* tract, extending into Westmoreland and Cumberland,† the old red sandstone and the limestone are found in repeated alternation with each other, and with slate clay; and the great coal formation is distinct, occupying the *eastern* quarter, and reposing immediately on sandstone and sandstone conglomerate with slate-clay. But in the *western* portion of the tract (where 21 beds of limestone are enumerated as alternating with the sandstone and slate-clay), discontinuous thin seams of indifferent coal are occasionally found, rarely exceeding one foot, or at most 20 inches, in thickness. Of six such seams of coal, which are noticed, the first (reckoning from below upwards) occurs in the interval between the 1st and 2nd beds of limestone; the second, between the 3rd and 4th limestone; the third and fourth between the 19th and 20th limestone; the fifth, between the 20th and 21st limestone; and the sixth, immediately below the 21st limestone. On the other hand, in the *northern* portion of the tract (distinguished as group

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* See Mr. Farey in the Philosophical Magazine, vol. xliii. p. 126 and 326; and Mr. Greenough's Geological Map of England and Wales.

† See the valuable papers of Mr. Winch and Prof. Buckland in vol. iv. of the Geol. Trans. which, taken conjointly, place the relations of that tract in a luminous point of view, and show that the series repose on transition, or on primary rocks. In this tract I coincide with Mr. Winch, in considering as one the groups marked No. 21 and 20 in Mr. Greenough's Map.

No. 21 in Mr. Greenough's Geol. Map), several seams of good coal, varying from a few inches to three feet nine inches in thickness, are found in direct alternation with the sandstone, limestone, and slate-clay.

General Remarks.—In the first floetz series of England and Ireland, trap, porphyry, or amygdaloid, are occasionally found; not only in separate association with the old red sandstone, with the limestone, and with the coal formation, but conjointly with all the three together. *Separately:* in the old red sandstone formation in Mellfell, Cumberland (Group, No. 22, with trap, No. 34, of G. M.); in the limestone formation in the county of Limerick, and in Derbyshire (Groups, No. 21 and 20, with trap, No. 33 and 32, of G. M.); and in the coal formation in Staffordshire (Group, No. 18, with trap, No. 31, of G. M.)* *Conjointly*, in alternation with the sandstone, limestone, slate-clay, and coal, in Northumberland (Group, No. 21, with trap, No. 33, of G. M.).

5. *Scotland.*—The intimate connection of the old red sandstone with trap, porphyry, amygdaloid, limestone, slate-clay, and coal, is also very striking in Scotland, all being found in that country in repeated alternation with each other, and presenting no very determinate order of succession, save that the lowest portion of the old sandstone formation constitutes in general the great foundation of the whole series. Dr. Boué, in his highly valuable work,† observes, that the tracts of the old red sandstone in that kingdom containing coal may be conveniently divided into a *lower* and an *upper* portion; the coal in the former appearing in inconsiderable beds, as pitch-coal, or occurring in the form of a black powder mixed with earthy particles, or as beds of anthracite, which are sometimes of greater thickness. But it is in the *upper* portion of the series that the true workable coal-fields, which form the great object of the miner, are chiefly found; and in this portion, certain quarters are distinguished by the absence of beds of limestone, by the abundance of coal and vegetable impressions, and by freshwater shells; circumstances, which are characteristic of the great coal fields of England, but which are of rare occurrence in Scotland. Of the general structure now noticed, as prevailing in the latter kingdom, Dr. Boué has given a clear exposition in the body of his work, beside detailed descriptions of very instructive sections in the notes appended to it.

General Remarks.—As in the tracts of the first floetz series, to which my preceding observations have been directed, the formations composing them correspond in general geological relations; so also they partake of similar mineralogical characters, and are distinguished more or less by the same organic remains.

The old red sandstone, wherever it comes in contact in those tracts with a transition, or with a primary country, is found to vary, more or less, in a corresponding manner, in its composition and structure. It may be said to be generally free from organic remains; and if such appear at all, they are almost wholly of vegetable origin, and chiefly found where the sandstone is adjacent to limestone, slate-clay, or coal.

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* See also Mr. Aikin's paper in vol. iii. of the Geol. Trans.

† Essai Géologique sur l'Écosse, par A. Boué, MD.; an instructive and compendious view of the geological structure of Scotland.

Of the organic remains met with in the carboniferous limestone, I confine myself chiefly to the notice of fish and trilobites, as being comparatively of rare occurrence in that formation. Impressions of fishes, accompanied with shells, are found in the limestone, which forms a part of the lower beds of the coal basin near Old Cumnock, in Ayrshire.* Small teeth of fishes occur likewise in the limestone that forms part of the upper portion of the coal series in Scotland.† And palates and bones of fishes are met with in the Bristol limestone,‡ which, with the intervention of a belt of sandstone and sandstone conglomerate, supports the contiguous coal field of Somersetshire and South Gloucestershire.

I found trilobites three years since in the Mendip limestone, and I am informed that they have been met with latterly in the limestone near Dublin. Mr. Greenough notices their occurrence, also, in the shale accompanying the limestone in Holy Island, on the Northumberland coast.—(G. M.)

Trilobites have been found likewise in the clay ironstone of the coal formation in Shropshire; besides terebratulites, and the productus scabriculus. Winged anomites are met with in the shale of the Killenaule coal district in Ireland; § and in that of Linlithgowshire, in Scotland, four species of orthoceratites are found, one of which accompanied by six other distinct species, occur, also, in the carboniferous limestone of that tract.‖ Orthoceratites, it is well known, occur in the Derbyshire mountain limestone; also, in that of Yorkshire, Mendip, and Bristol, and in Ireland. And ammonites are met with in limestone nodules, and in clay ironstone, and terebratulites in limestone and sandstone, all imbedded in the slate clay of the coal formation in England.**

Let us now direct our attention to the Continent.

In Germany.—In that country, also, it may be remarked of the old red sandstone in general, that, wherever it occurs, its composition is found to vary, accordingly as, in the course of its extent, it comes in contact with, and reposes upon, differently constituted transition or primary tracts. The formation may also be said to be generally free from organic remains. And in examining the observations of M. Freiesleben and other writers on the forest of Thuringia, on Mansfeld, Thuringia, and the circle of the Saale;* and of Baron von Buch,† and M. von Raumer, on Lower Silesia, the county of Glatz, and part of Bohemia and Upper Lusatia,‡ in all of which it occurs of great extent and thickness, we shall find that in these tracts the old red sandstone, carboniferous limestone, and coal formation, display relations very analogous to those existing in Scotland.

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* Dr. Boué, Essai Géologique sur l'Ecosse, p. 172. † Ibid. p. 195.

‡ Dr. Bright in Geolog. Trans. vol. iv. p. 198 and 200.

§ Geol. Trans. vol. v. ‖ Dr. Fleming in *Annals of Philosophy*, vol. v.

** Sowerby's Mineral Conchology.

1. *Forest of Thuringia.*—The old red sandstone (*rothe todtliegende*) is here, also, in frequent association with porphyry and trap, and contains, likewise, incidentally, thin interrupted beds of limestone, carbonated shale, or coal shale (kohlenschiefer of M. Freiesleben), and coal itself (pitch coal, anthracite, and slate coal), with slate-clay, bearing the usual vegetable impressions; the subordinate beds being of uncertain extent, while the whole series are found more or less in alternation. The coal shale is remarkable for containing occasionally not only vegetable impressions and petrified shells, but likewise impressions of fishes, e. g. near Goldlauter, and in other places; where it is not unfrequently metalliferous, bearing grey copper ore, with pyrites of copper, arsenic, and iron, more or less argentiferous, and in sufficient quantity, to have become the object of mining operations. The coal, also, in the Allthal, near Kleinschmalkalden, contains impressions of fishes, mytilites, and myacites, and bears radiated iron pyrites, copper pyrites, and galena; while the coal, near Mannebach, passed, both on the line of range and of dip, into firm shale, containing numerous vegetable impressions, occasionally accompanied by pyrites, and massive galena, yielding two ounces of silver in the quintal; and the coal itself, it is said, was not wholly free from silver.

2. *Mansfeld, Thuringia, and the Circle of the Saale.*—All the details given by M. Freiesleben of these tracts, including the districts of Sangerhausen, Stollberg, Ihlefeld, and Anhalt Bernburg, tend to prove:

That the old red sandstone (rothe todtliegende) of those quarters contains, incidentally, subordinate beds, or masses, of porphyry, trap, and amygdaloid, and beds of limestone, slate clay, coal shale, and coal;

That the old red sandstone, porphyritic and amygdaloidal trap, and coal formation, are in direct and immediate association with each other; while the limestone occurs only in inconsiderable beds in the old sandstone, rarely exceeding ten feet in thickness. In this limestone have been observed terebratulites and discites;

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* See the elaborate work of M. Freiesleben, entitled, "Geognostischer Beytrag zur kenntnis des Kupferschiefergebirgs, 1807—1815, 4 vols. 8vo.; the "Geologische Beschreibung des Thüringer Waldgebirges, by M. Heim; and the observations of MM. Voigt, von Schlotheim, von Hoff, &c.

† Von Buch. Geog. Beob. auf reisen durch Deutschland und Italien, 1802, vol. i.

‡ Von Raumer. Das Gebirge Nieder-Schlesiens, du grafschaft Glatz, und eines theils von Böhmen, und der Ober-Lausitz, geognostisch dargestellt, 1819.

That both the coal shale and coal form beds of greater or less continuity, but appear to close and terminate, both in the line of range and of dip. These beds contain occasionally pyrites of copper and iron, blende, and galena, in membranous flakes, or disseminated; and near Löbegün, Wettin, and Dölau, they are said to have contained nodules of cobalt also.

The principal coal deposit of these countries is found in the tract of the Petersberg, situated between Halle on the S, Wettin on the W, and Löbegün and Kathau on the N. And combining the observations of M. Freiesleben with the communications of MM. Schulze and von Veltheim, it appears clearly, that the coal beds at Kathau underlie porphyry, at Löbegün repose on porphyry, near Wettin again underlie porphyry, while at Raunitz, south-east of Wettin, they repose on porphyry. Again, to the east of Brachwitz, they appear to underlie porphyry; while between Halle and Giebichenstein, they are distinctly imbedded in porphyry.*

3. *Lower Silesia, County of Glatz, and Part of Bohemia and Upper Lusatia.*—The old red sandstone (rothe sandstein of M. von Raumer) is here found in two distinct tracts; one being situated to the north of the Riesengebirge, and the other being spread over the southern side of the Riesengebirge and Eulengebirge.

The northern formation reposes wholly on primary tracts. It contains, and alternates with, subordinate beds of porphyry, trap, amygdaloid, and limestone; but only in one case has a trace of coal been found in it; namely, near Merzdorf on the Bober.

The southern formation of old red sandstone is similarly constituted, yet contains numerous beds of coal † in its northern and eastern quarters, where it adjoins the Eulengebirge, and where it reposes partly on transition, and partly on primary tracts; but the western quarter of the formation, which rests upon the primary tract of the Riesengebirge, appears to be free from coal. In the carboniferous portion, the red sandstone conglomerate and sandstone alternate with slate clay, coal, porphyry, basaltic trap, amygdaloid, and limestone.* Of these, the sandstone conglomerate, sandstone, slate clay, coal, and limestone are, in general, very distinctly stratified; the porphyry very seldom; and the basaltic trap and amygdaloid apparently never. The sandstone, particularly the conglomerated, passes, not unfrequently, through a hybrid compound of sandstone and porphyry into porphyry containing pebbles, and thence into true porphyry. The porphyry and trap are found in distinct beds, but they constitute also high, extensive, mountain masses, of a nearly round, or of an elongated form, which have a sensible influence on the stratification of the adjacent country. Thus we find that around the great porphyritic masses of the Hochwald and Hochberg, situate between Waldenburg and Gablau, the coal seams and concomitant strata, the lower portion of which underlie, and the upper repose on the porphyry, undergo great inflections, conforming in a great measure to the outline of the masses, which they thus enclose.†

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* Upon the mutual relations of the old red sandstone and coal formation in general, M. Freiesleben sums up his opinion in the following words: "That a portion of the real coal formation is found subordinate to the rothe todtliegende, can no longer admit of doubt. Even Lehman maintained that position, and it has been subsequently elucidated and confirmed by the observations of Madihn, Gerhard, Lasius, Karsten, von Buch, Heim, and von Hoff. MM. von Schlotheim and von Hoff are even disposed to incorporate the whole of the coal formation (the coal rocks, properly speaking), with that of the rothe todtliegende. On the other hand, M. Voigt, and some other mineralogists, have deviated from the position of Lehman. For my own part, I consider the rothe todtliegende and the true coal tracts (eigentliche steinkohlen-gebirge, composed of silioeous conglomerate, coal sandstone, shale, bituminous shale, and coal), as two, nearly allied, and yet distinct, formations; although I am also convinced that seams of coal are found subordinate to the rothe todtliegende."—(See vol. iv. p. 170—173, with the notes.)

† Von Buch. Geog. Beob. vol. i. p. 81—90, and p. 101—103.

In the carboniferous portion of the tract, there appears to be an uninterrupted connexion and interstratification of the whole series. But M. von Raumer, in considering the formation in a general manner, has distributed it into three principal masses.

1. The *lowest*, composed of red conglomerate, alternating with red sandstone, slate clay, coal, and porphyry.
2. The *intermediate*, red sandstone alternating with claystone, porphyry, basaltic trap, and amygdaloid.
3. The *upper*, red sandstone with subordinate beds of limestone.

It is to be observed, however, that the limestone is found also in five places within the division No. 1, and in several places within the division No. 2; so that, in fact, it is more or less distributed through the whole formation, having been observed altogether in 22 different places,‡ and forming beds which seldom exceed 10 to 14 feet in thickness.§ On the other hand, beds and masses of porphyry, basaltic trap, and amygdaloid, are found also within the division No. 3.

In the slate clay, which accompanies the coal, the usual impressions resembling ferns, reeds, &c. are met with; and similar impressions occur in the slate clay which accompanies the black limestone of Ottendorf, &c. And in the limestone itself, which is most commonly of a reddish-grey cast, impressions of fishes have been observed in two places, in a low and a high part of the formation; at Kunzendorf near Neurode, and at Rupersdorf between Friedland and Braunau.*

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* The porphyry of this tract was formerly considered as more ancient than the old red sandstone formation; but the researches of M. von Raumer, combined with the actual experience of the mining officers of Waldenburg, have proved that the porphyry in question is of contemporaneous origin.

† See the map of the coal fields near Waldenburg, in M. von Raumer's work, reduced from that constructed by the mining officers.

‡ See p. 106.

§ Von Buch, Geog. Beob. vol. i. p. 105.

In the Netherlands.—As the coal tracts of Germany, now noticed, bear a close analogy in structure to those of Scotland; so do those of the Netherlands correspond with some of the coal fields in England.

In the 24th volume of the Journal des Mines, M. Omalius d'Hallöy has taken an instructive view of the mineral constitution of the adjacent parts of France and the Netherlands; and M. Clere has given in the same work (Aug. 1814) a valuable account of the Eschweiler coal basin; from which it appears, that the coal tracts of the Low Countries are generally confined by a transition country, composed chiefly of associations of clay slate with quartz rock, greywacke, and limestone. In these tracts, the old red sandstone is of partial distribution, and the carboniferous limestone, when it appears, reposes either on that rock, e. g. on the Meuse below Namur, or upon the transition country just noticed, e. g. in the Eschweiler district. And here, also, as in many of the English coal fields, the immediate foundation of the coal basin is formed by a broad belt of sandstone and sandstone conglomerate, interposed between the coal formation and the carboniferous limestone.

If the preceding view of the mutual relations of the first floetz group in the British Isles and on the Continent tend to establish, in this instance, the proposition of a general order, combined with a variation in detail; this is no more than what is confessedly known to prevail also in the primary and transition formations. The same doctrine appears, also, to hold good when we extend our views to the higher and later portions of the geological series. And if we consider the British Isles, though occupying but a small space on the surface of the globe, as containing within their compass, the chief exemplifications of its general structure, in fact, a type of its mineral conformation, which is, probably, not far removed from the truth; the following distribution of the formations, in which the floetz constitute four principal series, may not appear inapposite, upon a due examination of their respective characters and more prominent relations.

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* In the Petrefactenkunde of Baron von Schlotheim, published in the year 1820, it is stated, that the author has recently met with pholadites, solenites, and venulites, in the coal formations of Germany. But the principal shells found in the coal tracts of that country, are, it appears, as in England, freshwater shells.

I. PRIMARY FORMATIONS.*

II. SECONDARY FORMATIONS.

A. Transition.*

B. Floetz: which, as characterized by particular minerals, or by predominant constituent masses, may be designated as

First Series.

1. Old red sandstone. 2. Carboniferous limestone. 3. Coal formation.	The great carboniferous tract.

Second Series.

1. Calcareous conglomerate. 2. Magnesian limestone. 3. New red sandstone.	The great gypsum and saliferous tract.

Third Series.

1. Shell limestone (lias and oolite). 2. Ferruginous and green sandstone and limestone. 3. Chalk.	The great shell limestone tract.

Fourth Series.

All formations of an era later than chalk, including the newest floetz trap formation of Werner.	The great trap and porphyry tract.

A more detailed exposition of the floetz series in the British Isles, compared with their equivalents on the Continent, is the following:

First Series

Has been already detailed.

Second Series, as comprehending the Formations of

In England.	*In Germany.*
1. Calcareous conglomerate and sandstone. Included in group No. 17 of G. M. being the lowest bed.	1. Conglomerate and marly sandstone, the *weissliegende* of M. Freiesleben. This is considered by that author as the lowest bed of the following formation.

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* I omit all detail of the primary and transition formations, as a fuller consideration of them does not enter into my present view.

2. Limestone, argillaceous, sandy, and magnesian. Included in group No. 17 of G. M.	2. The Lower limestone formation of M. Freiesleben, comprising In the *Lower Portion,* *a.* Marl shale, partly bituminous and metalliferous, *copper shale.* Bituminöser mergelschiefer, or *kupferschiefer,* and dachschiefer. *b.* Compact limestone, somewhat argillaceous. Zechstein (*toughstone*). In the *Upper Portion,* *c.* Porous sandy limestone, somewhat bituminous. Rauhwacke, rauhstein (*roughstone*). *d.* Earthy and slaty swinestone. Asche and stinkstein. *e.* Clay and marly clay. *f.* Lower or cavernous gypsum, and rock-salt. Schlotten gyps and steinsalz. As *substitutes* of the preceding appear partially in some tracts; *a.* Cavernous limestone. Höhlen-kalk, rauh-kalk. *b.* Ferriferous limestone. Eisen-kalk.
3. New red sandstone, and claymarl, with gypsum and rocksalt. Group, No. 16 of G. M.	3. The clay and sandstone formation of M. Freiesleben, consisting *principally* of sandstone and clay, with beds of slaty sandstone and roestone; but including, also, *incidentally,* beds of limestone and marl, loose sand and conglomerate, and gypsum and rocksalt. *The second floetz sandstone formation* of Werner.

Third Series, comprehending the Formations of

In England.	*In Germany.*
1. Shell limestone. *a.* Lias limestone. Group, No. 15 of G. M. *b.* Oolite. Groups, No. 14 up to No. 9 inclusive, of G. M.	1. Upper, or shell limestone formation of M. Freiesleben. *Muschelkalkstein* of Werner.
2. Ferruginous and green sandstone and limestone. Groups, No. 8 to No. 6 inclusive of G. M.	2. Quader and Pläner sandstone and limestone of Werner, Hausmann, von Schlotheim, von Raumer, &c. The *third floetz sandstone formation* of Werner.
3. Chalk. Group, No. 5 of G. M.	3. Kreide of ditto.

Fourth Series, comprehending the Formations of

In England.	*On the Continent.*
1. Plastic clay. Group, No. 4 of G. M. 2. London clay. Group, No. 3 of G. M. 3. Freshwater limestone. Group, No. 2 of G. M. 4. Upper marine. Group, No. 1 of G. M.	In the Paris basin, and other parts of France, Netherlands, Switzerland, Germany, Italy &c.
5. Wood coal. 6. Newest floetz trap, porphyry, and syenite, of Werner. In the north of Ireland, the Western Isles, and mainland of Scotland, overlying both primary and transition tracts, as well as the floetz of the first, second, and third series. Perhaps, also, in part, in the north of England.	In Bohemia, Saxony, Silesia, Auvergne, Italy, &c. In the same position, and, also, overlying and alternating with floetz formations of the fourth series, e. g. in Auvergne, and the north of Italy.*

To complete our view of the existing state of the crust of the globe, we must also notice

III. PYROGENEOUS FORMATIONS.

Mineral masses affected by the operation of subterraneous fire;

a. The pseudo-volcanic; e. g. collieries in a state of combustion. England and Germany.

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* See, for instance, Dr. Daubeny in the Edinb. Philos. Journal, vols. iii. and iv.; and Ebel's Alpen-Bau, vol. i.

b. The volcanic; active, in Iceland, the Andes, &c.; extinct, in Auvergne, Banks of the Lower Rhine, &c.

Their further consideration is not connected with my present subject.

In noticing in detail the second and third floetz series, I shall confine myself in a great measure to those formations in Germany, which came under the more immediate view of M. Freiesleben, and are described by him; my observations on which may prove acceptable to the English geologist, by affording him an opportunity of drawing for himself a parallel between them and the British. The latter I shall here merely indicate, since they form a field, of which we have hitherto received no full account, and my own researches have been merely of a local character. Their complete investigation, however, would probably bring to light many instructive facts, and tend more fully to establish their correspondence with the German.

(*To be continued.*)

ARTICLE II.

On Thomsonite,[*] *a new Mineral Substance.*
By P. Squires, Esq.

(To the Editor of the *Annals of Philosophy.*)

SIR, *Norwich, Aug.* 26, 1821.

I BEG to hand you a few particulars respecting a mineral lately found in this neighbourhood by myself, which does not seem to be described in any of the works on mineralogy with which I am acquainted, and request, if convenient, you will give it publicity in your pages. I am, Sir, with respect,

Yours obediently,

PAUL SQUIRES.

The colour is snow-white, and sometimes yellowish-white, and in the massive specimens passes into asparagus green. It occurs crystallized in four-sided prisms, about an inch and a half in length, terminated in flat irregular sided pyramids, levelled at the extremities. The lateral planes are longitudinally striated. The crystals are imposed on the same mineral, massive, which is uneven and sparry.

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[*] In vol. xvi. p. 193, it will be seen that this name has been already given to a mineral substance. It will be better, therefore, if Mr. Squires would designate the mineral found by him by some other term.—*Ed.*

The lustre is vitreous, inclining to resinous externally, internally splendent. The fracture is fibrous; cross fracture small and earthy; in the massive inclining to splintery.

It is transparent in the largest specimens, which are rare; the massive translucent on the edges.

It scratches rhomb spar, and is scratched by fluor. It is easily frangible.

Specific gravity very low; as near as I could calculate only about 2·15.

Under the blowpipe, it, with difficulty, fuses into a white enamel. It slowly effervesces with the acids, and loses 38 per cent. in weight, leaving six parts of an insoluble powder, which has the properties of silicia. The solution holds lime and magnesia, which, when separated, as correctly as I could, by Dobereiner's method (*Annals*, Nov. 1818), which is to precipitate with carbonate of ammonia, &c. yielded 30 parts lime, and 19 parts magnesia. A few grains of salt of potash I unfortunately neglected, which makes the analysis more imperfect than I could wish. Meanwhile, it stands thus:

Lime	30
Magnesia	19
Carbonic acid	38
Silicia	6
Potash, water, and loss	6
	100

It occurs in a chalk pit belonging to Mr. Fountain, of this city, near Little Catton.

This mineral not being described in any system, I have ventured to designate it *Thomsonite*, in honour of the illustrious chemist and mineralogist of that name, and Professor in Glasgow.

I shall endeavour to forward you a specimen, by the hand of a friend, at an early day for your examination P. S.

Article III.

Tables of Temperature, and a Mathematical Development of the Causes and Laws of the Phænomena which have been adduced in Support of the Hypotheses of "Calorific Capacity, Latent Heat," &c. By John Herapath, Esq.

(*Continued from* p. 211.)

When, to gratify my friend the Rev. Mr. Trimmer, I first undertook to write the present paper, I intended to confine myself to a simple development of the mathematical laws of the phænomena I had in view, and to close them with a few observations on the hypothesis of "Latent Heat;" an hypothesis for the welfare of which the extent and soundness of this gentleman's Newtonian views, will not allow him to have that regard and solicitude which many philosophers have so anxiously, and, perhaps, so laudably, displayed. Since that period circumstances have occurred to induce me to interrupt the series of propositions with a popular view of my ideas on the changes of state, the nature of vapours, and the phænomena of evaporation. By this course I hope to avoid as much as possible the charge of clouding philosophical views with mathematical formulæ. Unfortunately the shortness of the time since this interruption has appeared necessary, has put it out of my power to make those extensive researches requisite to complete my theories, or even to give my ideas that elegance of arrangement alike due to the dignity and the utility of the subject. For any blemishes of this kind which may appear, I trust, with the candid and liberal, I shall be entitled to some indulgence; but however much such imperfections may detract from the merit of this part of the paper, and however great a portion of indulgence I may on this account, in the opinion of some, stand in need of, I hope it will not be discovered I have any reason to claim an allowance for glaring errors, visionary views, unphilosophical deductions, or palpable and unpardonable inconsistencies.

Theory of the Causes of the Changes of State in Bodies, with an Elucidation of some of the concomitant Phænomena.

Whenever a body changes its state from a fluid to a solid, or from a solid to a fluid, a new arrangement of the component particles, or atoms, generally takes place; in the former case, there is commonly a further aggregation of the atoms; and in the latter, a division of the particles. How this aggregation or division may be effected, and to what cause it may be owing, I have briefly hinted in my late paper. If the primitive atoms of a body were spherical, the body could never exist but in the fluid or gaseous state; no degree of cold however great could affect the arrangement or manner of intestine motion of the particles, or produce any degree of relative fixity in the parts: such a body would be a perfect fluid. But if the atoms or particles have a certain fitness or adaptation of figure, the cohesive tendency, as I have shown in the above paper, will be greater than if the atoms or particles were spheres; and in proportion to this adaptation will be the cohesive tendency or adherence of the atoms or particles. If the particles of a solid, by whose vibrations the temperature of the solid is measured, be composed of atoms or other particles of different degrees of adaptation, then, because the cohesive tendency of the parts of the particle will always remain nearly the same, the intensity of the collision of the particles on one another, arising from the temperature, may be so increased as to overcome this adhesion of the parts of less adaptation, and, consequently, disunite them from the other portions of their respective particles. The parts of, perhaps, several contiguous particles disunited in this way, having but a small individual motion, may, with a slight adaptation, easily unite, and form a new particle or particles; so that, perhaps, out of four, five, or six particles, there may be formed five, six, seven, or more. Should it happen, as it is most probably the case, that the cornered irregular parts are disunited, the particles afterwards having a more spherical form, as well as a greater latitude of motion from their diminution of size, will have a greater freedom of motion among each other; and, consequently, compose even at the same temperature a body of a more soft and fluid nature. If the disunited parts happen to be sufficiently great, the remaining parts may be sufficiently spherical, and from their diminution have so increased a latitude of motion as to compose a body nearly perfectly fluid, such as water, mercury, &c. In other cases, this disunion of parts may not absolutely take place. The fluidity may result entirely from the latitude of motion being gradually augmented, until it be great enough to enable the particles in their vibrations to exceed the influence of the irregularity of figure in restraining a perfect freedom of motion. Such bodies will have their rigidity gradually diminished until they become quite fluid. Tallow, wax, glass, pitch, tar, &c. seem to be bodies of this kind. Other bodies again may have their rigidity gradually diminished, and at certain temperatures disunions beside take place; by which view of things there seems to be the greatest scope for expounding every possible variety of phænomena. Amidst all these different bodies, however, it is manifest that any one body will always have the same degree of liquidity, or softness, at the same temperature; and, therefore, whether the given temperature be produced from a higher or a lower temperature, the liquidity at that temperature will be the same, agreeable to experience.

When certain parts of the particles are incapable of remaining in union beyond a certain temperature, if the solid at a much loss, the particle's force of unity is still further weakened; and thus matters will proceed until the particle be completely dismembered, and its parts arrogated by some of the others.

During all the time of the solidification, it is plain the temperature of the body will remain constant; for as soon as the least solidification ensues, the temperature in the immediate neighbourhood of it rises. This excess being quickly abstracted by the contiguous particles, a diminution, another solidification, and a consequent rise of temperature, ensue. And in this way the temperature of the body in the parts of the solidification will oscillate a trifle; but the general temperature cannot descend below the point of solidification.

Though it be generally true that during the congelation the temperature of the body remains stationary, yet it is possible for the body, under peculiar circumstances of figure in the particles, to be cooled down considerably without solidifying. Suppose, by way of example, the particles are of that figure that it is only in some parts of them the superior adaptation for the parts of the dismemberable particle exists. Then if the vibrations of the particles are such that the unitable parts do not come in contact, an increase or diminution of temperature will dilate or contract the paths of vibration; but if there be no agitation to disturb the relative movements of the particles, it will not affect the figure of their paths, or the manner of their collision; and, consequently will not so soon dispose them to submit to those changes which produce solidification. If, however, during the time the temperature is beneath the point of congelation, any agitation calculated to disturb the relative motions of the particles is given to the body, the unitable parts will be more likely to be brought nearer together; and thence, of course, a rapid solidification will take place, and the whole temperature be raised to that of solidification, provided the previous temperature be not too low. That the temperature will rise to this, if the agitation be sufficient, and no higher, and that such a quantity only of the fluid will be solidified as will enable it to rise to this point are manifest; for if by a greater solidification it should rise higher, a liquefaction, until the body was reduced to this temperature, would be the consequence; because the body cannot retain its solidity beyond this temperature, as I have before shown. Moreover, if the agitation be sufficient to give all the particles a like chance of bringing their unitable parts together, it follows that if the temperature be not so low that the excess resulting from the entire solidification of the fluid would be incompetent to raise the temperature to the point of solidification, a sufficient quantity at least will solidify to bring the temperature of the whole to that of solidification. And because there is no reason why a solidification should take place in one part of the fluid more than in another, the whole mass will either be equally and completely solidified, or the solidification will be uniformly partial throughout the fluid. In the latter case, the solidified part will exhibit a porous imperfect kind of solid, like the spungy mass observed by Dr. Thomson.

While its temperature is below the degree of solidification, if any other motion be impressed on the fluid, not calculated materially to disturb the relative motions of the particles, the body will obviously, notwithstanding this motion and the lowness of its temperature, still retain its fluidity. Hence the reason of the phænomena observed by Sir Charles Blagden, Phil. Trans. 1788, namely, that a stirring of water cooled below 32° Fahr. does not cause it to congeal; while a tremulous motion does. For by the one, the parts of the fluid merely slide over one another without, perhaps, scarcely affecting the relative motions of the particles; and by the other, a succession of irregular impulses is propagated throughout the fluid, and, consequently, to every part of it, by which the relative motions of the particles cannot fail to be considerably disturbed.

Sir Charles also tells us, that opaque bodies floating in the water cause it to shoot into crystals when only a few degrees below the freezing point. This probably arises from the heterogeneous figure of these bodies producing a constant irregularity in the motions of the aqueous particles, and thus disposing the body more readily to solidify. The same philosopher likewise informs us, that too sudden a cooling down of the water will cause it to freeze. Now if we conceive when the temperature is above the freezing point, the particles to have a latitude of motion which would render their excursions wholly independent of the influence of the figure of the particles, these excursions in this case may be attended with a sort of rude irregularity which would not affect the fluidity under such circumstances, and which might be gradually constrained to within more regular and uniform limits by a slow and gentle abstraction of temperature, so as not to destroy the fluidity; but which, if too suddenly acted on by a rapid preposterous deduction of temperature, would, for the reasons we have already given, cause an instant solidification.

A piece of ice thrown into water cooled below 32°, Blagden observes, causes it immediately to shoot out into crystals. This seems to be owing to some peculiarity in the arrangement or figure of the icy particles, aided by the lowness of the temperature, which produces a dismemberment in the first instance of the adjacent divisible particles, and thence by a rapid communication, a dismemberment of the more distant ones. The precise way in which this is effected, a greater variety in the experiment is necessary to determine. Many notions might indeed be easily formed of the manner in which it could be done; but as experiment cannot be brought to their support, I prefer not detailing them.

We have now traced the principal phænomena attending the changes of state from the solid to the fluid, from the fluid to the aëriform, and *vice versâ;* and we have seen that in general, changes from the solid to the fluid, and from the fluid to the aëriform state, are accompanied with an increase in the numeratom as referred to equality of weight, and, consequently, with a diminution of temperature. On the contrary, in changes from the aëriform to the fluid, and from the fluid to the solid state, a diminution in the numeratom, as referred to equality of weight, will generally be the case, which will be productive of an increase of temperature. These conclusions, though holding pretty generally, are by no means to be considered as universal laws. Instead of an increase of temperature in the condensation of vapours, and the solidification of fluids, or a diminution in the vaporisation of fluids, and the liquefaction of solids, we may either have the very contrary, or no change of temperature at all, and by no means violate our general views. We have already shown that the liquidity of bodies depends on two circumstances; the sphericity and the extent of the aberrations of the particles. Were the particles perfectly spherical, the body would be perfectly fluid; and even if they were not, a perfect fluidity may be obtained by only giving them such a range of vibration as would carry them beyond the irregularities of their figure. Therefore, if we suppose a decomposition to take place in the particles, in the manner we have before described, a recomposition may be so effected as to make the new particles not only more spherical, but, perhaps, of an equal or less number; in which cases a perfect fluid may be formed with an equal or even a higher temperature, than the solid had from which it was derived. For the converse reasons a solid may be formed from a fluid of an equal or an inferior temperature. I cannot say that I can call to mind any particular instance of this kind, nor will the short time I have to prepare for the press allow me to make any researches; but surely nothing can be a greater proof of the beauty and probability of a theory than this very extensive generality I have shown it to possess.

Some idea of the cause and manner of union between the particles of aëriform bodies, and the consequent condensation will be given in a subsequent part of this paper, on which account I shall not stop here to detail it; but we may anticipate that the particles of two distinct gases may have a very great adaptation of figure, though the particles of either gas alone have little or no adaptation. The particles of two such gases mixed together would speedily form a union either with or without condensation, according as this union does or does not dispose the particles to a further aggregation. But if it should not so dispose them, an increase of temperature and specific gravity will be the consequence. An example of this kind of union is found in the formation of carbonic acid gas from a mixture of carbonic oxide and oxygen, though the combination requires the assistance of an electric spark, which may be considered an artificial means of more readily disposing the particles to unite. In other respects, the phænomenon precisely follows the consequences of our theory. Not only is there an increase of temperature, followed afterwards by an increase of specific gravity, but, what is a still more remarkable confirmation of our views, the "specific heat," as it is called, of the product, carbonic acid gas, is less than that of either of the component gases, when referred to equality of weight, and greater when referred to equality of volume, just as it should be according to our theory.

If the particles of the new gas have a proper irregularity and adaptation of figure, another union may take place; and for like reasons, perhaps, this may be followed by others until the particles become too massy for the body to continue in a gaseous state, which will be productive of an immediate condensation into a fluid or solid. In general, this condensation will, on account of the aggregation of the particles, be accompanied with an increase of temperature, which is usually the case in gaseous condensations. But if there be a greater adaptation between certain parts of the different aggregated particles than between those parts and the rest of the particles to which they are respectively united, no sooner will the gas be condensed than a decomposition will take place, and the more unitable parts will form new particles; and still the body will remain in a solid or fluid state. By this means it may happen that the number of particles in the solid or fluid may, instead of being considerably less, be, perhaps, greater, equal to, or but very triflingly less, than the sum of the particles in the two gases previous to condensation. In such a case, the temperature of the solid or fluid result would either be less, equal to, or but a very trifle greater than that of the gases. Though with our views it is manifestly possible, there is no instance that I know of in which a fluid or solid product from gaseous condensation has an inferiority of temperature. We have, however, converse cases, in which aëriform bodies are produced from solids with a considerable elevation of temperature that amount to very little less than a direct proof. Gunpowder is a well-known example of this kind. Therefore, if we knew how to reduce the air which is derived from the explosion of gunpowder to its solid state, we should have an instance of the conversion of an air into a solid accompanied with a loss of temperature. Such an experiment, though at present impracticable, is not impossible in idea; might it not, therefore, be esteemed an elucidation of the accuracy of our general principles?

Instances likewise occur where the rise of temperature is so little as to be comparatively insensible in respect of other similar phænomena. For example, when ammoniacal gas and muriatic acid gas are mixed together, they produce a solid, and yet a very inconsiderable rise of temperature takes place. "Sulphurous acid gas and ammonia, sulphuretted hydrogen gas and ammonia, both of which condense into a solid salt when mixed in equal volumes, occasion a still smaller elevation of temperature."* These, and other instances of the kind, which it is easy to adduce, though confessedly of the utmost difficulty to be accounted for on the calorific hypothesis, are very easy consequences of the theory I have given.

Again, let us conceive a solid or fluid composed of particles, the parts of which, to put the theory under the most unfavourable circumstances, we will suppose, are united by their most adaptable sides. Then though in any particle these contiguous sides be the most favourable for union, and, consequently, the other more exterior sides be less favourable, yet such may be the relation of these contiguous sides that their mutual adaptation may be much less than the adaptation they have for the like, or probably interior sides of similar or other parts of other particles. While the body, therefore, continues in its present state, these sides of superior adaptation in the different particles cannot come in contact, because they are already in contact with other parts of their respective particles; and thus, when the particles meet, are turned from, rather than towards, one another. No decomposition and other combination can, consequently, in this state of things take place. But if we suppose the particles of a small part of the body to be by any means decomposed into an air, the lowness of the temperature of the new particles, as we shall hereafter show, arising from the decomposition, may very much contribute to unions between the most adaptable parts. Nor is there any reason why, if the figures of the particles favour it, that the unions may not be carried to a length sufficient to make the number of the particles of the decomposed mass less, and, consequently, their magnitude and temperature greater in the gaseous than they were in the solid state. This being the case, the gaseous particles may strike with a force sufficient to decompose another such a mass, or, perhaps, more of the body; and this being in like manner gasefied will produce a similar effect on another portion, and so on until the whole body is converted into gas with a rapidity, perhaps, in appearance, equivalent to a total instantaneous explosion, and with a rise of temperature proportioned to the aggregation of the particles. Thus the explosion of a single particle may be sufficient to explode the whole mass, however large a quantity it may be. The confirmation which is given of these views in the explosion of gunpowder, fulminating argentum, fulminating aurum, &c. are too trite and familiar to need any comment.

Whatever differences exist between powders of this kind may be explained on the simple principle of adaptation. The less the adaptation of the corpuscular parts in the solid, and the greater in the consequent gas, the more easily the body will explode.

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* Thomson's System of Chemistry, i. 75.

This explanation will hold good equally in the general outline, whether the explosion leaves a residuum, as by Mr. Robins's experiments it appears the firing of gunpowder does, or whether the body in the explosion combines with any part of the atmosphere, which, it would seem by the increased weight of the gaseous product, aurum fulminans does. The only objects I have had in view are to show that a solid may be converted into a gas having particles of a greater magnitude, and that the explosion of one small part of the body is enough to explode the whole; both of which objects are independent of the above peculiarities.

In all cases I think it will be found, where bodies explode in this way by communication, that the explosion will be accompanied with an increase of temperature, unless any deposition might operate to the contrary, of which it would be difficult to form an idea without experiment.

From the views I have taken, it appears that all changes which produce an increase of particles out of the same quantity of matter, must be attended with a diminution of temperature; and all changes which produce a diminution of particles in the same quantity of matter, with an augmentation of temperature. Conversely all changes of temperature arising from any change in the constitution or nature of the body, are accompanied with an aggregation or decomposition in the particles; that is, with a chemical change; for chemical changes do not consist in any alteration of the component atoms, but merely in new arrangements and associations of them. But notwithstanding such changes of temperature (which, to distinguish them from natural elevations and depressions, may be called corpuscular changes of temperature) are invariably attended with a chemical change in the body or its particles, it is not conversely a universal rule, that chemical changes produce a corpuscular change of temperature. In other words, the rule that has been generally given by chemical writers; namely, "that all chemical changes produce an alteration of temperature," is not a law of nature, nor necessarily true in all cases; but rather an extensive case of a universal rule. If the change has not increased or diminished the number of particles in the body, or the sum of them in the bodies, if more than one body, the temperature will not be affected, however great an alteration may have taken place in the colour, density, and chemical properties, of the body or bodies. But the case of a perfect equality of temperature before and after the change requires so nice a balance in the numbers of the particles, and so small a deviation in the ratio of these numbers would produce so considerable a difference in the previous and subsequent temperatures, that it is not surprising an instance of perfect equality has, perhaps, not been discovered. By our theory, a difference of one particle in a thousand at the temperature of ice melting, would be enough to change the temperature of the result a degree of Fahrenheit; and one in a hundred would make near 10°. At the higher temperatures the same difference in the numbers of the particles would occasion a still greater difference between the temperatures.

Notwithstanding we can hardly, under such circumstances, expect to, and cannot, that I know of, instance a case of perfect equality in the previous and subsequent temperatures, where a chemical change has been effected, yet there are many phænomena which prove its possibility. Thus, to go no further than the cases I have cited, in gun and fulminating powders, there is a conversion of solids into airs accompanied with a very considerable increase of temperature; while, in general, such conversions are productive of very considerable diminutions of temperature. Now if the conversions of water, vinegar, alcohol, &c. into airs are attended with diminutions of temperature, amounting to between 800° and 1000° of Fahrenheit, as philosophers tell us, what reason can be assigned why the gasefyings of those powders, which, in the general view of things, appear to be parallel cases, should be attended with so great augmentations of temperature? Do not such phænomena prove that they are the demonstrated extremes of an equally possible mean? Besides in some of the other cases that I have mentioned, for instance the condensation of sulphuretted hydrogen gas and ammonia into a solid, the rise of temperature is so small compared to that which results from the condensation of aqueous vapour into a fluid only, that it might almost be neglected. Here then are examples of gases being converted into solids with scarcely any elevation of temperature; while,,by the ordinary examples of the generality of other similar phænomena, the elevation ought to have been upwards of a thousand degrees. In the absence of a direct particular case, such facts are surely as decided a proof as can be expected of the accuracy of our general conclusion; namely, that it is *possible* for a chemical change to take place without any alteration of temperature.

Many other examples might easily be advanced of the facility with which the various phænomena connected with a change of state flow from our principles. We might also extend the same views to the phænomena of combustion, and might show that they all flow from the same simple principle;—adaptation of figure in the particles of the supporters and combustibles, and inadaptation in the supporters and incombustibles; but as I have already said much more of my general views of the phænomena of the change of state than is suited to a paper of this kind, I shall reserve a further detail of my inquiries into this part of the subject to another opportunity.

*Theory of the Nature and Laws of Vapours.**

The intimate physical cause of the conversion of fluids into vapours being closely connected with the cause of the reflection of light at the anterior surfaces of bodies, the limits of the present paper would not allow me to develop it in a manner suitable to its extent and importance; for which reason I have thought it better to omit touching on the subject in this paper altogether. And as it might have seemed strange to treat of the cause of condensation without explaining that of vaporization, and I perceived it would be necessary to say something of the nature and laws of vapours, I preferred deferring my observations of condensation to this place when they might serve to throw some light on the nature and laws of vapours.

Were the particles of an air sufficiently small, it would, perhaps, be impossible by any means, short of total abstraction of motion or temperature, to produce a union or condensation when they meet; for the force of condensation diminishes as the magnitude of the particles diminishes, while the force to resist which is as the temperature may remain the same. But one of the chief causes of condensation is the irregularity and adaptation of figure. Spheres of all figures are the least adaptable, and the most efficient to resist a union. When bodies with irregular figures meet, the chance is many to one against their striking each other in a line passing through their centres of gravity; consequently, the collisions will generate a whirling kind of motion about these centres of gravity, which will occasion the bodies sometimes to strike with a greater, and sometimes with a less, than their mean force. By this means it must happen, notwithstanding the mean motion of the particles be the same, that they will recede with a less force after some collisions than after others, and hence will be less able to resist a union in those cases than in these. This inability to oppose a union will likewise be increased or diminished according as the most adaptable sides are turned towards or from one another at the times of the collision, and according as the particles are then moving towards the same or opposite parts. Let us, therefore, conceive that at a certain temperature the force of collision is sufficient to overcome the most favourable circumstances of union, but at a less temperature it is not. Then at all higher temperatures the air would preserve its gaseous property, but at inferior temperatures, when the concomitant circumstances were favourable, the particles would not be able to recede with a force equal to that resulting from the adaptation of their striking parts, and they would, consequently, unite; and this union successively running through the parts of the air will compose an air with less in number, but more massy particles. The particles of this new air being greater would, at the same temperature, have a less velocity, which would alone, if the figures of the particles disposed them to it, tend very much to promote a further aggregation. Independent of this, the superior action of the particles resulting from their increased masses, if aided by a sufficient mutual adaptation of figure, would produce other unions, though it should be at a considerable elevation of temperature; and if the resulting particles have yet adaptation enough, the unions may be carried to a still greater length. However, it may happen that after one or two unions only the masses of the particles may become too great to enable them to be reflected with a sufficient velocity to preserve their aëriform property, and then of course a condensation will immediately follow. Under any circumstances, if the temperature and successive figures favour it, a condensation will be the consequence. This is the most general view that can well be given of the cause of condensation. There are, however, other circumstances which might contribute towards, or effect it, that I may hereafter explain.

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* This theory will receive a further elucidation when I come to speak of the phenomena and laws of evaporation.

If this theory of vapours be granted, it follows that the whole difference between gases and vapours consists in the figures of their component particles, and that, therefore, vapours unconnected with their fluids at all higher temperatures than those at which they would condense, have the same laws of expansion and contraction as gases, which agrees with the experiments of Mr. Dalton; for he has found that their elasticity unconnected with the fluids from which they are formed, increases or diminishes with their temperature; and that their bulk undergoes precisely the same increase which air does under the same circumstances and change of temperature. These two laws, especially the last, are a decided demonstration of the views I have developed, and authorise, therefore, the admission to vapours of all the laws of gases I have demonstrated in my last paper.

When any number of equal volumes of different gases having no chemical action are mixed at the same temperature, the elasticity of the compound mass reduced to the same volume is equal to the sum of the elasticities of all the component gases. This theorem, which experiment has confirmed, is not demonstrated in my last paper; but it is a very easy consequence of what I have there delivered. Hence vapours above certain temperatures being of the same nature, and following the same laws of dilatation as gases, the same law of elasticity will likewise hold good in a similar mixture of any number of vapours, or of any number of vapours and gases, provided no chemical action takes place. This conclusion has been experimentally proved both, I believe, by Mr. Dalton and M. Gay-Lussac.

From the experiments of Mr. Sharpe and of Mr. Southern, it would seem that the specific gravity of steam in connexion with its water is nearly in proportion to its elastic force. Though this might appear to hold very nearly in their experiments, it cannot, however, be perfectly accurate, if Mr. Dalton's laws of vapours be correct. According to the laws of gaseous bodies, I have heretofore demonstrated, and which, both by the experiments of De Luc* and myself, have been shown to accord with phænomena, the numeratoms, or which are in this case the same, the specific gravities of different quantities of the same kind of air, are as the elasticities directly, and the squares of the temperatures inversely; and, therefore, the specific gravities cannot be proportional to the elasticities, unless the temperatures are equal. Even taking it on the old theory with the Fahrenheit temperature, Sharpe and Southern's conclusion, and the preceding laws, will be found equally inconsistent. By the gaseous laws, which all philosophers admit, the specific gravities of any portions of the same air at equal temperatures are as the elasticities; and it has long been found that the increments of Fahrenheit temperature would in either portion produce nearly proportional decrements of specific gravity. Consequently under no circumstances whatever can the specific gravities and elasticities be proportional if the temperatures are unequal. It may, perhaps, be thought that the connexion of the vapour with the fluid and its successive generation might make the circumstances vary from the ordinary laws of gases; but this cannot be the case; for the conclusion I have drawn is so general and independent that it could not be affected by such circumstances. The true solution of the problem seems to be, that Messrs. Sharpe and Southern's experiments, though embracing a long range on Fahrenheit, were not carried to that extent to make the ratio of the true temperatures very sensible; which, combined with the errors inseparable from such difficult and delicate experiments, rendered it impossible for them to appreciate any little anomalies. Viewed in this light, these experiments become a further confirmation of the theory I have given. The following table exhibits, according to the views of Messrs. Sharpe and Southern, the specific gravities of aqueous steam calculated from the elasticities observed by Mr. Southern, at a few temperatures, compared with those resulting from the theory I have given; the specific gravity of common air at 60° Fahr. being 1000.

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* From some circumstances which have lately come to my knowledge, I feel it necessary to observe, that in quoting philosophers I have nothing to do with their public or their private feuds. With many of the eminent living authors, I have not the honour of an acquaintance, and with those who have been dead, even but a few years, I could have no connexion. The expression of my opinion has been uninfluenced by private feeling, nor have I in my observations been actuated by partiality or prejudice, much less by any desire to give offence.

Temp. Fahr.	Elasticities in inches, of mercury by Southern.	Sp. gr. of steam by Southern and Sharpe.	Sp. gr. of steam by new theory.	Differences of results.	Ratios of results.
212°	30°	0·4545	0·4545	0·0000	$\frac{1}{1}$
250·3	60	0·9090	0·8591	0·0499	$\frac{660}{698\cdot3}$
293·4	120	1·8180	1·6184	0·1996	$\frac{660}{741\cdot4}$
343·6	240	3·6360	3·0315	0·6045	$\frac{660}{791\cdot6}$

From this table it is plain, how very trifling an error it would require to make any of Messrs. Sharpe and Southern's experiments coincide with the theory I have advanced. Even in the last case, an error of less than one-sixth only, which on this subject is considerably within the probable errors of observation, would be sufficient to make the two numbers coincide. How desirable, therefore, would a careful repetition of such experiments be? But to give the finishing stroke to the theory of vapours, there is wanting a correct and extensive set of experiments on the temperatures of the boiling points of different fluids under various compressions. Such experiments as these carefully made for a great range of temperature, and connected with the expansions of the fluids under similar circumstances, would be of more service towards perfecting the laws of cohesion, the theory of the steam engine, and, it appears to me, even the laws of the reflection and refraction of light, than almost all the experiments that have yet been made valuable as they are. De Luc, Betancourt, Shuckburgh, and, more lately, Gay-Lussac, have set philosophers some fine examples in this interesting and useful part of experimental physics; but still their experiments have not that extent which would enable the analyst to investigate the true laws that connect the temperature of ebullition with compression; and unhappily the little discordancies found in their results are more calculated to display the difficulty of the inquiry, than to render the experiments useful to the philosopher.

To this same irregularity of figure in the vaporous particles another remarkable property of vapours seems to be attributable. "It is well known," says Dr. Thomson, p. 74, of vol. i. of his System of Chemistry, Sixth Edition, "that the condensation of vapours is greatly assisted by pressure; but the effect of pressure diminishes as the temperature of vapours increases." Now from the views to which I have alluded, and which probably at some future period I may unfold, it appears that if the vapours be condensed to a certain degree, the irregularity of the corpuscular figures, and their adaptation, or a considerable deviation from sphericity, will very much contribute to unite the particles and produce condensation; but the more violent the collisions, or the higher the temperature, the more vigorously, of course, will these efforts to condense be resisted.

Thus the difference between condensible and permanent airs, as I have before hinted, may be entirely owing to the figures and, perhaps, the size of the component parts; and hence we have the greatest latitude for explaining the different phænomena. An extreme minuteness and a perfect sphericity, or a total inadaptation in the particles, would probably resist every effort to produce condensation unless the temperature could be entirely destroyed. Hence the reason that common air, which is composed of oxygen and nitrogen, could not be condensed though the compression has been carried so far as to make the air heavier than water. The particles of oxygen and nitrogen may not indeed be exceedingly small, nor their figures be in anywise spherical, but their inadaptation may be so great as to render condensation extremely difficult.

Ammoniacal gas aided by pressure and a low temperature has been condensed to a liquid, but by no methods yet devised have the other gases been separately condensed. Now these things may entirely be owing to a certain mutual adaptation of figure in the one instance, and a much less, or, perhaps, scarcely any at all, in the other cases.

Though the particles of an aëriform body may have among one another little or no adaptation, and hence render the body incapable of being separately condensed, it may however, happen, that the particles of two such bodies may have so great an adaptation as to render it difficult to keep them together without condensing. An instance of this kind is found in sulphurous acid gas and sulphuretted hydrogen gas; for when two in volume of the former are mixed with one in volume of the latter, the mixture slowly condenses into a solid. The same phænomenon occurs in a mixture of ammoniacal and muriatic acid gases, except that the solidification in this case is almost instantaneous.

Theory of the Influence of external Pressure on the Temperatures of Ebullition in Fluids, and Liquefaction in Solids.

I have already stated that I cannot fully enter into my views of ebullition on account of their being so closely connected with a subject which would lead me to, indeed, some very important, but more extensive investigations than I have at present leisure to attend to. It is of course without the pale of my views now to explain in detail the cause of the effect which pressure has on the temperature of ebullition. Lest, however, the general theory I have given should appear defective by the omission of one of the most difficult and interesting parts of it, I will just glance at the leading feature of ebullition, and reserve a detail of my views of the minutiæ to another opportunity. I have shown that the irregularity of figure in the particles of vapour is the cause of its condensation at all temperatures beneath that of its generation;

and from the views which I have in reservation, it appears that the same irregularity of figure will be one cause of condensation when the compression is sufficient to induce a certain degree of density. Hence, therefore, the density being the same, the compression will be the greater the greater the temperature; and, therefore, at a greater temperature a greater compression will be needful to produce condensation. But if a greater temperature in the vapour require a greater compression to convert it into a fluid, so, conversely, a greater temperature in the fluid will require a greater compression to prevent its being converted into vapour; and, consequently, the greater the compression, the greater will be the temperature requisite to produce ebullition.

This conclusion will be true for all fluids whose vaporisation is attended with a division in the particles; and, therefore, for all fluids with which we are at present acquainted; but it seems possible to devise cases in which it would not, perhaps, hold good. However, as such cases have not yet occurred, and probably never will occur, in nature, it would be useless to detail them. We may hence consider it a general law *that the temperature of ebullition of all fluids is increased with an increase, and diminished with a diminution of compression; and, consequently, that at all seasons the temperature of ebullition of any fluid will be the same, the external pressure being the same.*

From the view I have taken of ebullition, it follows, that the increments of the squares of the true temperatures of ebullition are proportional to the increments of the compressions; and, therefore, the increments of the Fahrenheit temperatures of ebullition are also proportional to the increments of the compressions. Consequently this being the case with every fluid, it would follow that the same excess or defect of compression will in all fluids produce the same excess or defect in the Fahrenheit temperatures of ebullition; and, therefore, the Fahrenheit temperatures of ebullition of all fluids, when the fluids are removed from the ordinary pressure of the atmosphere to a vacuum, will be lowered the same number of degrees, which agrees with the experiments of Prof. Robinson. These inferences, as I have already mentioned, are drawn from a general view of the principal cause of ebullition, and do not, therefore, include the minor circumstances; and hence cannot be expected to agree mathematically with phænomena. From the views indeed to which I have alluded, I find that the Fahrenheit temperature of ebullition ought to increase and decrease more rapidly than the compression, a circumstance which is in unison with the experiments of De Luc, Betancourt, and Shuckburgh.

Philosophers have long observed that different fluids boil at different temperatures. This phænomenon evidently depends on the different magnitudes of the particles and the different degrees of the adaptation of their parts. Other things being alike, the temperature of ebullition will be the greater the greater the particles, and the greater the force which is requisite to decompose them, or the greater the adaptation of their parts; and *vice versâ*, the less these two circumstances, the less will be the temperature of ebullition. With this view our theory and experiment will enable us to determine many interesting things respecting the figure and size of the component particles of different fluids; but of such speculations I may say something hereafter.

Though the temperature of ebullition in any fluid must necessarily be subject to the influence of external pressure, the same does not hold good with the temperature of the liquefaction of solids. In fluids we may conceive the temperature at which the particles would partially decompose much beneath that at which ordinary ebullition takes place. At all temperatures, therefore, higher than that of corpuscular decomposition, the ebullition is restrained solely by the pressure. We may form an idea how pressure in this case prevents ebullition, by considering, that as decomposition produces a great diminution in the temperature, the elasticity of the aëriform product would be very small; and, therefore, if a conversion of a part of the fluid into vapour could take place, yet from the great diminution of elasticity, the external pressure would instantly compress it to a density equal to, or greater than, the condensible density to which I have before alluded; and, therefore, condensation will immediately follow. Hence when the temperature is above that of corpuscular decomposition, and beneath that of ebullition, the efforts of the fluid to convert itself into vapour are instantly counteracted by the external pressure producing too great a contraction in the vaporous product. Beautiful instances of the counteracted efforts to vaporisation may be seen in the subsidings of the unbroken tumefactions of water just before ebullition. Here by some rapid accessions of temperature, the lower parts of the water suddenly vaporise, tumify the surface, and then, being too weak to sustain the superincumbent pressure, recondense before their levity can bring them to the surface. As the temperature rises, the strength of these vaporisations increases, until it be sufficient to resist the atmospheric pressure; and then the phænomenon terminates in ebullition by the boisterous escape of the vapour through the surface. On the contrary, as the temperature diminishes, the energy of the vaporisations decreases, and the surface becomes less ruffled, until it gradually settles into an apparent calm. During all this time, however, and even for some time after the tumefactions have apparently subsided, the same phænomena of vaporisations and condensations, there is every reason to believe, continue to take place, though not in a manner so tangible to the senses. In fact, as the same causes exist, the same phænomena of vaporisation and condensation must invariably ensue at every temperature above that of ebullition in vacuo, which indeed might be made evident in the tumefactions by only lessening the pressure. To two causes, therefore, the temperature of ebullition is owing, one corpuscular decomposition, which in the same fluid would of itself always produce an ebullition at the same temperature; and the other, the force of compression. The liquefaction of solids, however, is controlled by one cause only; namely, corpuscular decomposition. Pressure, in this instance, can have little or no influence; for as pressure cannot increase or diminish the individual intensity of collision, which is exclusively due to the temperature, and can besides have no effect on the facility of decomposition, which is in this instance likewise exclusively due to the temperature and adaptation of the parts; nor, moreover, on the state of the fluid product, which could never be changed by mere steady pressure whatever little influence it may have on the volume, we cannot, therefore, infer, that any sensible difference would be produced by external pressure alone, however great it may be, in the temperature of the liquefaction of solids. This inference, which agrees with the experiments of our ablest philosophers, is another beautiful instance of the accordance of phænomena with legitimate deductions from our general theory of the universe; and, I believe, our attempts to draw it are the first that have been made to unravel the causes of a phænomenon, which becomes the more singular and difficult when contrasted with its vacillating collateral—ebullition

(To be continued.)

Article IV.

Historical Sketch of Electro-magnetism. (With a Plate.)

(Continued from p. 200.)

The results obtained by M. Oersted were immediately repeated and confirmed by a great number of philosophers in various places. Of these no one was more active than M. Ampere in varying experiments, making new ones, and applying theory to them. That philosopher read a paper to the Academy of Sciences at Paris on Sept. 18, in which he proposed a theory that reduced all the magnetic phenomena to effects purely electrical, and in many subsequent writings advanced further arguments, both experimental and theoretical, in support of it. I am desirous, however, at present, rather to mention the *facts* as they were discovered than the theories attached to them: in the first place, because they are of the most importance; and in the second, because there is no danger of attributing the theories to any but those from whom they originate.

The facts discovered by M. Ampere, though not numerous, are of great importance. At a meeting of the Royal Academy on Sept. 18, he described an experiment proving that the voltaic pile itself acted in the same manner as the wire, connecting its two poles; and produced an instrument which, at the same time that it proved this action, was shown to be of great use in experiments, on currents of electricity. This was merely a magnetic needle, but from the uses to which it was applied was called a galvanometer. When placed near a pile, or trough, in action, having its poles connected either by a wire, or by introducing them into one cell, it immediately moved, becoming obedient to the battery in the same manner as to the connecting wire; and the motions were such as if the battery were simply a continuation or part of the wire. In consequence of this action, the needle becomes an instrument competent to indicate that state of an active voltaic pile, and the wire connecting it, which is supposed to be occasioned by currents of electricity, and in which only, magnetism has yet been discovered.

On Sept. 25, M. Ampere announced the new fact of the attraction and repulsion of two wires connecting the poles of a battery; and showed, that the magnetic needle which had previously been used to prove the magnetic attractions and repulsions of the wire, could be replaced by another connecting wire like the first. This discovery seemed to liberate the phenomena of magnetism from any peculiar power resident in the magnet, and to prove its production by electricity alone. When by Oersted's discovery it had been shown that a wire connecting the poles of a voltaic battery would act on a magnet, attracting and repelling it, just as another magnet would do, it was fair to assume that the wire possessed the powers of the magnet it supplied; and when the second magnet was replaced by another connecting wire, as in Ampere's experiment, and the powers and actions still remained as before, it was perfectly correct to consider these powers and actions as magnetical; so that it became evident that magnetism could be exerted independent of magnets, as they are usually called, and of any of the means of excitation usually employed, but wholly by electricity, and in any good electrically conducting medium.

The phenomena with two conductors situated between the poles of the battery are as follows: When they are parallel to each other, and the same ends of them are similarly related to the battery; i. e. when the supposed currents existing in them are in the same direction, then they attract each other; but if the opposite ends be connected with the battery, so that the currents conceived to exist in them are in opposite directions, they repel each other. If, also, the one being fixed, the other be moveable, and the currents be sent, or the connections be made in opposite directions, then the moveable one will turn round until they are in the same direction. The contrast between these attractions and repulsions, and those usually called electrical, are very striking. These take place only when the circuit is completed: those only when it is incomplete. The attractions take place between the similar ends of the wires, and the repulsions between the dissimilar ends; but the electrical attractions take place between dissimilar ends, and the repulsions between similar ends. These take place in vacuo, but those do not. When the magnetic attraction brings the two wires together, they remain in contact; but when electrical attraction brings two bodies together, they separate after the contact.

These experiments are varied in several ways by M. Ampere; and the apparatus with which they were made appears from the plates and description published to be very delicate, ingenious, and effectual. The general results drawn up by M. Ampere himself from them are: 1. That two electrical currents attract when they move parallel to each other, and in the same direction; and repel when they move parallel to each other in contrary direction; 2. That when the metallic wires traversed by these currents can only turn in parallel planes, each of the currents tends to direct the other into a situation in which it shall be parallel, and in the same direction; 3. That these attractions and repulsions are entirely different from the ordinary electrical attractions and repulsions.

On Sept. 25, M. Arago stated to the Royal Academy of Sciences that he had ascertained the attraction of iron filings by the connecting wire of the battery exactly as by a magnet. This fact proved not only that the wire had the power of acting on those bodies already magnetized, but that it was itself capable of developing magnetism in iron that had not previously been magnetized. When the wire in connexion with the poles of the battery was dipped into a heap of filings, it became covered with it, increasing its diameter to the size of a goose quill; the instant the communication was broken at either pole, the filings dropped off; and the instant it was re-established, they were re-attracted. This attraction took place with wires of brass, silver, platina, &c. and was so strong as to act on the filings when the wire was brought near them without actual contact. It was shown not to belong to any permanent magnetism in the wire or filings by the inactivity of both when the connexion was not made with the battery; and it was proved not to be electrical attraction by the connecting wire having no power over filings of copper, or brass, or over saw-dust. When soft iron was used, the magnetism given was only momentary; but on repeating the experiment with some modification, M. Arago succeeded completely in magnetising a sewing needle permanently.

The theory which M. Ampere had formed to account for the magnetic phenomena of magnets by electrical powers only, assumed that magnets were only masses of matter, around the axes of which electrical currents were moving in closed curves. This theory led him, when informed by M. Arago of his experiments, to expect a much greater effect if the connecting wire were put into the form of a spiral, and the piece to be magnetised were placed in its axis. According to the theory, in a needle or magnet, pointing to the north, the currents were in the upper part from east to west. In consequence of these expectations, MM. Ampere and Arago made experiments with spirals or helices, and the results are mentioned in M. Arago's paper, on the Communication of Magnetism to Iron Filings, published in the Annales de Chimie, xv. 93; so that probably the experiments date from Sept. 25, though there is no date to the paper.

On twisting a wire round a rod, it may be made to pass either in one direction or the other, giving rise to two distinct but symmetrical helices, which have been named by botanists *dextrorsum* and *sinistrorsum.* Though their diameters be equal, and the spirals which compose them have equal inclinations, yet they can never be superposed; for however they are turned about, their direction is the same. The dextrorsum, or, as we may call it, the right helix, proceeds from the right hand downwards towards the left above the axis; the tendrils of many plants exhibit instances of it, and it is almost exclusively used in the arts: the sinistrorsum, or left helix, proceeds from the left hand downwards towards the right above the axis.

Having made some of these helices, one was connected by its extremities with the poles of a voltaic battery, and then a needle wrapped in paper placed within it; after remaining there a few minutes, it was taken out, and found to be strongly magnetized; and the effect of a helix above that of a straight connecting wire was found to be very great.

Then with regard to the position of the poles in the magnetised needle, it was found that whenever a right helix was used, that end of the needle towards the negative end of the battery pointed to the north, and that towards the positive end toward the south; but that with a left helix, that end of the needle towards the positive, pointed north; and the other end, south.

In order to establish this point, the connecting wire was sometimes formed into one helix, sometimes into two or three, which was readily done by twisting it round a glass tube, or rod, first in one direction, then in another; and when needles previously inclosed in glass tubes were then placed in these helices, the magnetic poles they received were always in accordance with the statement just given. In one case, also, where the connecting wire had been formed into three consecutive helices, the middle one being of course different to the other two, a single piece of steel wire sufficiently long to pass through all three of them being inclosed in a glass tube was placed within them. On being again removed, and examined, it was found to have six poles: first, a north pole, a little further on a south pole, then another south pole, a north pole, another north pole, and at the further end a south pole.

The direction and constancy of the poles given to the needle by the helices will be directly seen to be a natural consequence of the invariable position of the needle to the connecting wire, pointed out in Oersted's experiments; for if a small portion of either of the helices, together with the needle magnetised by it, be compared with fig. 1, 2, or 3 (Pl. IX), they will be found to be represented by them. Thus in fig. 8, or 9, which represents the helices and the needles in them, every part of the helices will be seen to cross the needles as in 1, 2, or 3; or if two of the glass diagrams, fig. 3, be put together with the lines representing the needles together, so as to imply but one, then the lines representing the connecting wires will also represent one round of either helix.

In the same paper, M. Arago also states, that when the connecting wire was perfectly straight, a needle placed beneath and parallel to it was not at all magnetised. He also states, that it sometimes happened, though not frequently, that the copper wire connecting the poles of the battery retained its magnetism for a few instants after the connexion had been broken; and also that M. Boisgeraud had observed the same fact with a platina wire. These wires, it is said, would sometimes take up iron filings, or even a needle, when separated from the battery, but the power soon disappeared, and could not be reproduced at will.

On Oct. 9, M. Boisgeraud read a paper to the Royal Academy of Sciences, containing the detail of numerous experiments, most of which, however, are variations of Oersted's first experiments. He remarked that connecting wires, or arcs, placed any where in the battery would affect the needle—a result that follows as a consequence from Oersted and Ampere's experiments. He notices the difference of intensity in the effects produced when bad electrical conductors were employed to complete the circuit —a difference which Oersted himself had pointed out in the case of water. M. Boisgeraud, however, proposes to ascertain the conducting power of different substances by placing them in one of the arcs, cells, or divisions, of the battery, and bringing the magnetic needle, or Ampere's galvanometer, towards another arc, i. e. to the wire, or other connecting body, used to complete the circuit in the battery. With regard to the positions M. Boisgeraud notices of the needle and wire, they are all confirmatory of Oersted's statements, and may be represented by the figures before mentioned.

On Oct. 9, M. Ampere read another memoir on the phenomena of the voltaic pile, and on the method he intended to pursue in calculating the action of two electrical currents. At this sitting, also, he showed the mutual action of two rectilineal electrical currents; i. e. of two straight portions of the connecting wires; for it appears that the phenomena of attraction, repulsion, &c. were first observed with spiral wires. These actions, however, are exactly similar; and the view already given of them, as it relates to straight wires, is consequently more simple than the description of the effects with spiral wires can be; i. e. considering it as a matter of experiment only, and not of theory.

In consequence of the view which M. Ampere had taken of the nature of magnetism as dependant simply upon currents of electricity, it became an important object with him to ascertain the action of the earth upon such currents excited by the voltaic battery; for from his theory (presently to be stated), he expected that it would be equally efficient in directing these currents as in directing those supposed to exist in the magnetic needle. After some trials, he succeeded in overcoming the obstacles to delicate suspension, contact, &c. and constructed an apparatus in which a part of the wire connecting the two poles of a battery was rendered so light and mobile as to move immediately; the connection was completed with the pole, and took a direction which, with regard to the earth, was always constant, and in accordance with M. Ampere's theory. An account of these experiments, with the apparatus used in them, was read to the Royal Academy on Oct. 30. The first consisted of a wire bent so as to form almost a complete circle of about 16 inches in diameter; the two extremities were made to approach, and were placed one just beneath the other; and being attached to two steel points, were connected by them with two little basins of platina containing mercury, fixed so as to receive them; only one of the points touched the bottom of the cup it was placed in; so that the friction was scarcely any, and the mercury secured a good contact. The cups were connected with other wires that passed off to the voltaic battery; so that it was easy to make this moveable circle connect either one way or the other between the poles; and being inclosed in a glass case, any movement it might receive was readily observable without danger of its resulting from any other cause than the electric action.

When the extremities of this apparatus were connected with the poles of a battery, the circle immediately moved, and after some oscillations placed itself in a plane perpendicular to the magnetic meridian of the earth; and on every repetition of the experiment, the same effect took place. The direction in which it moved depended upon the way in which the connexion had been made with the battery; and if it be assumed that there is a current passing through the wire from the positive to the negative end, the curve so arranged itself that that current always passed downwards on the eastern side, and upwards on the west. This circle moved round a perpendicular, and, therefore, only represented the direction of the magnetic needle: in order to represent the dip, a wire was formed into a parallelogram, and being fixed to a glass axis was suspended by fine points, and connected as before so as to move round an horizontal axis; then this axis being placed perpendicular to the magnetic meridian, and the wires being connected with the poles of a battery, the parallelogram immediately moved towards a position in the plane perpendicular to the dipping needle; when the communication was broken, it returned towards its first position; and when renewed, it resumed the second, evidently indicating the magnetic influence of the earth over it. In consequence of the difficulty of placing the centre of gravity in the centre of suspension, and keeping it there, this conductor did not take its position exactly in a plane perpendicular to the dipping needle, but approached towards it till in equilibrium between the magnetic and the gravitating power of the earth.

On Oct. 30, MM. Biot and Savart read a memoir to the Academy of Sciences, the object of which was to determine the law by which a connecting wire acted on magnetised bodies. Small rectangular plates, or cylindrical wires, of tempered steel were made magnetical by the double touch, and being then suspended by silk worm threads were placed in different positions with, and at different distances from, the wire connecting the poles of the battery. The terrestrial magnetism was sometimes combined with that of the wire, sometimes opposed to it, and sometimes neutralised by the vicinity of another magnet. The different positions of equilibrium, and the number of oscillations of the needles, were then observed, and data gained, by which MM. Biot and Savart were conducted to the following result, which expresses the action exerted by a molecule of austral or boreal magnetism, placed at any distance from a very fine and indefinite cylindrical wire, rendered magnetic by the voltaic current. Let a line pass from this molecule perpendicularly to the axis of the wire, the force which draws the molecule is perpendicular to this line and to the axis of the wire; its intensity is reciprocal to the distance. The nature of the action is the same as that of a magnetised needle placed on the surface of the wire in a direction determinate and constant in its relation to the direction of the voltaic current; so that a molecule of boreal magnetism and a molecule of austral magnetism would be drawn in different directions, though constantly according to the preceding expression.

Having succeeded in magnetizing iron and steel by the wire discharging the voltaic apparatus, M. Arago was led to expect the same effects from common electricity; and on trial found the results to be the same. He announced this fact verbally to the Royal Academy on Nov. 6, stating that he had produced all the phenomena in this way that he had observed in using voltaic electricity. No account of these experiments has, I believe, been published, but it is easy to conceive the general way in which they would be formed. They are very important, as identifying voltaic and common electricity, though few, I believe, still retain doubts on this point, and, also, as proving the magnetic phenomena not to depend upon this or that mode of exciting the electricity, but constantly to accompany it when moving.

In consequence of M. Ampere's theory, which attributed the powers of magnets to electrical currents, and also of the views taken of the manner in which it was supposed that currents of electricity in the connecting wire induced currents in steel bars placed near them, as in M. Arago's experiments; it was earnestly hoped and expected that such an arrangement might be made of magnets, wires, &c. as to produce the decomposition of water, or some other electrical effects; for as electricity produced magnetism, it was considered that magnetism might produce electricity. Many arrangements were made of magnets together, and of wire about and round magnets; and at first it was stated that electrical effects, such as the decomposition of water, attractions, &c. had been produced; but on Nov. 6, M. Fresnel, who had been very earnest in his endeavours to obtain these effects, allowed to the Royal Academy that the appearances were not such as to authorize the conclusion, that any certain effect had been obtained.

On the same sitting, also (Nov. 6), M. Ampere noticed an effect produced by the connecting wire bent into a helix. This may be easily understood from considering that the direction of the magnetic power is always perpendicular to the conducting wire. When, therefore, the conducting wire is parallel to the axis of the helix, the power is perpendicular to that axis; when the wire forms a circle round the axis, in a plane perpendicular to it, the power is in the direction of the axis; but when, as in the helix, it passes round the axis in a direction intermediate between parallelism and perpendicularity, the direction of the power is of course inclined accordingly. In this case the power may be considered as composed of two portions, one perpendicular to the axis, the other parallel to it. As M. Ampere considered magnets to be assemblages of currents perpendicular to their axes, he wished, in his imitation of them, to do away with that effect due to the extension of the wire in the direction of the axis of the helix, and succeeded in this by making the wire at one end return through the helix so as not to touch it in any part; for in this position, its magnetic effects being contrary to those belonging to the length of the helix, and also near to them, they neutralized or hid each other. An imitation of a magnet was now made by forming a helix, and making the wires at the two extremities return through the centre of the helix half way, and then pass out upwards and downwards, so as to form a perpendicular axis on which the whole might move. The extremity of a battery being connected with these two ends of the wire, the helix became magnetical, and was attracted and repelled by a magnet precisely as a real magnet would have been.

MM. Buch, of Frankfort, in repeating Oersted's experiments, Oct. 22, added nothing new to them; but the apparatus they employed was so simple and efficacious that they will make the experiments come within the observation of those who may otherwise think them too difficult to be easily performed. One was simply a platina crucible with a bent plate of zinc connected with the bottom, and then passing off from the side, and turned round till it dipped into the crucible, fig. 10. When diluted acid was put into the crucible, the apparatus acted powerfully on the magnetic needle. Another apparatus was formed from a small gilt spoon having a bent slip of zinc attached to the handle, and dipping into the bowl of the spoon. It acted very powerfully. A third was a needle formed of a piece of zinc and a piece of silver in the manner of a simple voltaic circle. This, fixed in cork, and placed in diluted acid, became obedient to the magnet when brought near it.

On Nov. 13, M. Lehot stated to the Academy of Sciences that, notwithstanding the results obtained by M. Fresnel, he was still convinced of the decomposing power given to iron wires by magnets, and quotes experiments he had made six years before by connecting iron wires to the poles of a magnet, and then immersing their ends in water. The south pole caused oxidation, the north pole preserved its wire bright; again in tincture of litmus; the south pole reddened the tincture; the north pole did not. There does not seem any reason to consider these experiments as decisive; and M. Lehot himself does not attach more importance to them than to those made 20 years ago by Ritter, and on the uncertainty of which M. Fresnel had sufficiently remarked.

On Nov. 13, also, M. Ampere read a note on the electrochemical effects of a spiral wire subjected to the action of the earth alone. The wire formed a helix round a paper cylinder, the axis of which was placed parallel to the dip and direction of the needle, the extremities were placed in a solution of common salt. In seven days gas appeared on both ends, but most on that answering to the negative end of the battery; the bubbles were displaced, but fresh ones appeared; the end remaining bright, while the other end became oxidized, and gave no more gas. On the whole, however, the experiment seemed uncertain, especially after what M. Fresnel had said; and M. Ampere himself said, that he still doubted as to the existence of the action.

On Nov. 16, a letter was read at the Royal Society from Sir H. Davy to Dr. Wollaston on the magnetic phenomena produced by electricity. The experiments detailed in it were most of them made in the month of October, and are of very high interest. The peculiar manner in which that philosopher compresses important and numerous facts into few words will seldom permit a condensed account being given of his papers. In the present case, however, there is no occasion to attempt such an account, since the paper itself has appeared in a late number of your *Annals*, and is to be found at p. 81 of this volume, where the deduction of fact from fact may be beautifully traced in it. The following is little more than an enumeration of the facts contained in it.

Viz. that the wire in connexion affected the needle as M. Oersted described; the effect was immediately attributed to the wire itself becoming a magnet; and this was instantly proved by bringing it near iron filings, which were attracted, and remained attached to it as long as the communication continued. This is in fact the same experiment as that made by M. Arago (see page 276) but it was made by the two philosophers independent of each other; and as no detail of M. Arago's experiment has as yet been published, the accurate description of Sir H. Davy's will be found highly interesting. This effect took place in any part of the wire, and any where in the battery. Steel needles placed on the connecting wire became magnetic; those parallel to the wire acted like the wire itself, those across it had each two poles; such as were placed under the wire, the positive end of the battery, being east, had north poles on the south of the wire, and south poles to the north; those needles above were in the opposite direction; and this was constantly the case, whatever the inclination of the needle to the wire. This position, it will be observed, is precisely that which has been referred to in the account of M. Oersted's experiments. On breaking the connexion, the steel needles across retained their magnetism, while those parallel to the wire lost it at the moment.

Wires of platina, silver, &c. in the same situation, were not rendered magnetic, except when, by accident, they formed part of the circuit. Whatever the position of the battery, or wire, the effect was the same. Contact was found not necessary: instantaneous effect was produced by mere juxtaposition, though thick glass intervened; filings arranged themselves in right lines across the wire on a glass plate held over it at a quarter of an inch distance. The effect was propostional to the quantity of electricity passing through a given space, without any relation to the metal transmitting it. Increasing the size of the plates proportionally increased the magnetic effects of the connecting wires. The wire connecting a battery of 60 pairs of plates did not take up half so much filings as when the battery was arranged so as to form 30 pairs of plates twice the size. The magnetic powers of the wire rose with its heat.

Considering that a great quantity of electricity was necessary to produce sensible magnetism, Sir H. Davy concluded that a current from the common machine would have no effect, while a discharge would; and this was found to be true; the poles of the needle magnetized being situated exactly as before. In these experiments a battery of 17 square feet, highly charged, being discharged through a silver wire, 1-20th of an inch in diameter, rendered bars of steel two inches long, and from 1-10th to 1-20th thick, so magnetic, as to lift up pieces of steel wire and needles, and the effect was communicated to needles at a distance of five inches from the wire, even when water, or thick plates of glass or metal intervene.

By these kinds of experiments it was found that a tube of sulphuric acid, one-fourth of an inch in diameter, did not conduct electricity enough to render steel magnetic; that an explosion through air made the needle placed transversely to it magnetic, though not so strongly as a wire would have done; that steel bars in the circuit, or parallel to it, did not become magnets; that two bars placed together across the wire, passing through the common centre of gravity, showed no magnetism after the discharge before they were separated, but exhibited opposite poles on separation.

One conclusion drawn by the author from these experiments is, that magnetism is produced whenever concentrated electricity is passed through space.

On arranging numerous wires in circles, and other directions around and about the discharging wire, it was found after the discharge, that all were magnetic, and the poles exactly as before expressed; so that the north pole of one needle was towards the south pole of the next, and in a constant relation to the course of the discharge, fig. 11.

The connecting wire being divided in one part into three, four, or more, by small wires, and the voltaic battery discharged through them, they were all found to be magnetic at once, and took up separate cylinders of filings; when the opposite side of two of these were brought together, the filings attracted each other. From this, it was expected that when the similar sides were brought together, the filings on them would repel each other. This was tried by two batteries arranged parallel, but in opposite directions. The filings on these connecting wires repelled each other; and connecting wires of platinum and fine steel wire without filings exhibit similar phenomena of attraction and repulsion. These experiments, it will be perceived, are similar to those made by M. Ampere on the attraction and repulsion of two connecting wires, or, as he calls them, two electrical currents, and prove the same thing. On placing straight pieces of platinum, silver, and copper wire, on two knife edges of platinum connected with the opposite poles of a battery, they were found to be attracted and repelled in directions according to what has before been said. Gold leaf made in the same way to form the connexion was found to be moved by a magnet.

A simple mode of making magnets is pointed out in this paper; namely, by fixing bars of steel across, or circular pieces of steel fitted for horse-shoe magnets, round the electrical conductors of buildings in elevated and exposed situations.

M. le Chev. Yelin appears to have discovered by accident that by placing a steel needle in a glass tube surrounded by a spiral of wire, along which either simple electrical sparks or discharges from a battery, were passed, the needle became magnetic. This result was published in the "Allgemeine Zeitung," of Dec. 2, 1820. Consequently, long after the previous experiments of the same kind, though without a knowledge of them. M. Bockman, in repeating and varying these experiments, increased the sine of the helix much beyond what it had formerly been made. When helices, varying from half an inch up to 13 inches in diameter, were used, the power of receiving magnetism in the steel needle placed in the axis of the larger did not seem at all diminished. With a helix 34 inches in diameter, the magnetism communicated was much less, and with one 84 inches it was hardly to be perceived. The surface used in these experiments was 300 square inches. It was found, on pursuing these experiments, that the needle without the helix was magnetised as well as that within; that after being fully magnetized, a continuation of discharges weakened its power; that five jars each, of 300 square inches, did not produce with repeated discharges much more effect than one of them; that a certain thickness of steel in the needle, or bar, was required to produce the greatest effect; and that on placing a steel needle within a tube of tinned iron; that in a glass tube, and a spiral round it, a discharge caused no magnetism in the needle, but made the tube magnetic; whereas, if the metal tube were lead, then the needle became magnetic.

M. Von Buch, of Utrecht, while engaged (Jan. 1821) in repeating the experiments of Oersted and others, obtained results according with them, except in one instance, of difference with Oersted. M. Oersted says, that "if the uniting wire be placed perpendicularly to the plane of the magnetic meridian, whether above or below it, the needle remains at rest, unless it be very near the pole: in that case, the pole is elevated when the entrance is from the west side of the wire, and depressed when from the east." M. Von Buch points out that this state of rest does not continue in two of the four positions of the wire. When the connecting wire is beneath the centre of the needle, and the positive current is from east to west, the needle remains unmoved. When the current is from west to east, it performs half a revolution. On the contrary, the connecting wire being above the current from east to west, makes the needle turn half way round; while that from west to east leaves the needle unmoved. M. Von Buch conceives the difference of his results and M. Oersted's to depend upon the superior power of his apparatus; and indeed it is sufficiently evident that the incompleteness of M. Oersted's results depended upon the weakness of his pile. The attractions and repulsions, or the elevations and depressions, he speaks of when the wire was brought near the poles proves the existence of that action which in M. Von Buch's experiments was strong enough to turn the needle round; and if the position of the wire and needle in these experiments be compared with the positions deduced from M. Oersted's experiments, fig. 1, 2, and 3, it will be found that in two of the cases, those pointed out by M. Von Buch, it was necessary a half revolution of the needle should take place to bring it into a state of equilibrium with the wire in those positions.

M. Von Buch, also, appears to have ascertained the effect of common electricity in producing magnetism without a previous knowledge of what had been done by others in that way, and succeeded in producing the effect by a smaller power than had before been used for that purpose. He found that a strong discharge was not necessary, nor even a Leyden phial; but, fixing a helix between the prime conductor of a machine and another insulated conductor, placing a steel needle in it, and then drawing sparks from the latter conductor, the needle became magnetic. One single turn of a machine, with two discs 18 inches in diameter, was sufficient to make the needle evidently magnetic.

In Italy, many experiments relating to magnetism by electricity have been made, and which, though new at the time to those who made them, had been previously made by others. A series was made between Jan. 6 and 18, by MM. Gazzeri, Ridolfi, and Antinori, at Florence. The results, which appear to me to be most interesting, are as follows: Needles placed in helices connected with the poles of the battery received their full magnetization in one minute. Needles on the outside of the helices would receive no magnetism, unless there was one or more also within, and then they became magnets with their poles in opposite directions to the poles of the inner magnet. The helix was changed into a square form, by having its wire wrapped round a parallelopiped; the magnetising effect remained the same. A needle and a long wire of platina were wrapped in a sheet of tin foil, and that part which contained the needle introduced into a spiral of copper wire; the circuit was then made by the platina wire without the copper spiral; being in connexion with either pole, the needle became magnetised. A spiral of copper wire with a needle in it was placed on the surface of a basin of mercury, and the mercury then made part of the circuit: the needle became feebly magnetic. Sparks from a common machine taken through a helix containing a steel needle made the needle magnetic. These philosophers appear to have found that the connecting wire placed in other parts of the battery than from end to end would not magnetize needles. There is, probably, some mistake in this.

M. la Borne, in repeating Arago's experiments, Jan. 8, varied the use of the helix by making it of iron, and putting it round the straight wire, through which an electrical discharge was made. The helix in this case became the needle to be magnetised, and it was found to be a strong magnet, the poles being in the positions so often referred to. Such a magnet is flexible and elastic, and may be doubled, lengthened, or shortened: on bringing the two poles together, its action on a magnetic needle was much diminished.

In a letter (without date) from M. Berzelius to M. Berthollet, published in the Annales de Chimie, for Feb. p. 113, an experiment is described which consisted in placing a thin leaf of tin, eight inches long and two inches wide, parallel to, and in the plane of, the meridian, and in that position connecting it with the elements of a voltaic circle. A magnetic needle brought near the lower edge of this plate was thrown 20° from the magnetic meridian. On moving it slowly upwards, it took its natural position, when level with the middle of the plate, except that it was raised at one end, and depressed at the other; and when near the upper edge, it moved 20° from the magnetic meridian in the opposite direction to what it did below. When the needle was moved up and down on the opposite side of the plate, the same deviation and effects took place, but in opposite directions. A small portion of the upper edge of the leaf was cut, and turned upwards, forming a projection above the edge. The needle brought within equal distance of this projection, and the edge, was more affected by the former than the latter.

Then using a square plate of tin, and forming the connexion at opposite angles, it was found on examination that the intervening angles acted more powerfully on the needle than any other parts —a circumstance which proves, M. Berzelius says, that the magnetic polarity of the current goes to opposite extremities, as happens with electric polarity, and in artificial magnets.

The tin band or leaf placed in a horizontal plane, and in the magnetic meridian, acted on the needle just as a wire would have done. The greatest deviation of the needle was immediately under or above the middle of the leaf, and the edges acted as in the former position. The positions assumed by the needles in these experiments is exactly what would be expected. The experiments receive all their interest from the way in which their maker applies them to support his particular opinion, and apart from that have not much new in them. M. Berzelius thinks that a round wire, when made the conductor, presents a more complicated case than when a square one, or a parallelopiped, is used. I shall endeavour to return, however, to the theory advanced by this philosopher presently.

M. Lehot's experiments (Feb. 27) are repetitions of what had been previously made, and are only intended to prove that a straight conductor can communicate magnetism in opposition to the opinion of the Marquis Ridolfi, who considered that it must be more or less turned round the needle to be magnetized.

M. Schweiger's experiments have nothing in them new after what has been said. I do not know their date, but they are inserted in the Bib. Univ. for March, 1821. The author made his connecting wire pass several times round the needle, producing in fact the same accumulation of effect as in the helix, and used only two plates, four inches square. The effects were very strong on the magnetic needle. M. Schweiger opposes M. Oersted's theory, and advances one of his own.

In the same number of the Bib. Univ. M. de la Rive describes the construction of two small apparatus, intended to show two of the experiments made by Ampere, the attraction of an electrical current by a magnet, and his artificial electro-magnet. The first is made of two slips, one of zinc, the other of copper, passing through a cork float, and connected above by a copper wire curved. When this apparatus is placed on the surface of dilute acid with the lower parts of the slips immersed, a voltaic combination is formed, which may be attracted and repelled by bringing a magnet in different directions towards the connecting copper wire above: fig. 12. The other is a zinc and copper plate, floated on a cork as before, but connected above by a helix. This helix is made by wrapping a copper wire covered with silk round a small glass tube; then slipping it off from the tube, making the ends of the wire return through the helix till near its middle, and then passing to the outside between the curves of the helix; they are then connected with the ends of the zinc and copper slips; and on the instrument being placed on the surface of acidulated water, the ends of the helix will be attracted and repelled, like the poles of a magnet: fig. 13. These apparatus are very simple, easily made, and effectual.

M. Moll, in three letters to the Editor of the Journal de Physique, the first dated March 23, the others without date, gives an account of some experiments made to ascertain the relative power of a battery consisting of many small plates, and one of two large plates only. The large apparatus consisted of a narrow trough of copper, containing a plate of zinc, presenting near four square feet of surface. The smaller apparatus was one consisting of plates, four inches square, put together in Dr. Wollaston's manner, with the copper round the zinc. With the large apparatus, M. Moll remarked, that the magnetic power was very great, when the connecting wire was of considerable thickness (2-10ths of an inch); but when a platina wire much smaller was used (1-100th of an inch), the power diminished considerably. With a copper cylinder, however, about one inch in diameter, the power was diminished. No chemical action could be obtained by this apparatus on making the connection with saline solutions, or tincture of litmus, though the magnetic effects were very powerful. In making the comparison between this apparatus and that with small plates and cells, 36 pair of the latter were taken; so that an equal surface of zinc was used in both instruments. These being put into action by the same acid, and the connecting wires being similar, that of the two plates made the needle deviate from 60° to 70° from the magnetic meridian; while that of the small plate battery made it deviate only 12°. The decomposing power of the small plate battery was very considerable, and M. Moll draws the conclusion, that the apparatus with cells produces intense chemical effects while its magnetic action is very little; while the single plates have scarcely any chemical effect, though their magnetic power be very great.

M. Moll also remarks, that the positive and negative state of his single plates is in opposition to the states of a battery of many plates, inasmuch as the zinc pole is negative, and the copper pole positive. He found the positions of the magnetic needle near his wire the reverse of what it was near the wire of a battery of many plates; for when the wire extended from the zinc pole, north, to the copper pole, south, a needle placed beneath turned towards the west instead of the east, and above in the contrary direction. The cause of this difference will be readily seen, when it is remembered that M. Moll was using the wire that connected the zinc and copper plates of the *same* pair, and not of *different* pairs; so that, in fact, his connecting wire was in the inverse direction of those used to connect between the poles of a battery with four or more plates. Hence the deduction, that the plates in the single pair battery are in an opposite state to those of the battery of many plates, is probably premature.

In endeavouring to increase the size of the plates, and yet preserve the instrument in a portable form, the plates were, at the suggestion of Lieut.-Col. Offerhaus, bent into a spiral form one within the other. This arrangement has been adopted some time since by Dr. Hare, of Philadelphia. The effects with this instrument were very powerful, and similar to those described above.

Though possessing such powerful apparatus, M. Moll could not succeed in magnetizing a needle, except by making the connecting wire form more or less of a spiral around it; and concludes from his experiments, that it is absolutely necessary that such spiral should exist to give magnetism. There can, however, be no doubt, particularly from the experiments of Sir H. Davy, that such spiral is not necessary, since a straight discharge transverse to a needle magnetized it, though many inches off. Neither does M. Moll admit of the magnetization of needles on the outside of spirals.

Some experiments made by M. Pictet, April 7, at Florence, confirmed the results obtained by M. Oersted.

M. Poggendorf's galvano-magnetic condenser is simply a helix placed perpendicularly, and having the ends of the wire forming it attached to a zinc and a copper plate, which are placed in dilute acid. When an unmagnetized needle is raised on a pivot into this helix so as to be perpendicular to its axis, it is said soon to become magnetic whatever may be the nature of the helix, or however connected with the plates, and to point north and south. It is to be observed, that the needle is not in this case, as in all the previous experiments, in, or parallel to, the axis of the helix, but is perpendicular to it. It is probable that it becomes magnetic by some indirect action of the apparatus.

Finally, a paper was read on July 5 to the Royal Society by Sir H. Davy on the magnetic phenomena produced by electricity, and their relation to heat occasioned by the same agent; but this has not yet been published, nor any account of it given to the world, so that I am unable to state what facts it may contain.

Such, Sir, is an imperfect account of the experiments made on this subject since Oersted's discovery, that I have been able to get access to. With regard to what had been done before that time, though many philosophers had dwelt on the relation of electricity to magnetism, and theorised upon it, yet very little else than opinion can be found in their writings. I cannot, I think, do better than copy the note at the commencement of Sir H. Davy's first paper, to show how little had been done at that time, and with that I shall finish this historical sketch of facts; and endeavour, in the remainder of this letter, to give a somewhat familiar account of the different theories of electro-magnetism that now exist.

"M. Ritter asserted, that a needle composed of silver and zinc arranged itself in the magnetic meridian, and was slightly attracted and repelled by the poles of a magnet, and that a metallic wire, after being exposed in the voltaic circuit, took a direction NE and SE. His ideas are so obscure that it is often difficult to understand them; but he seems to have had some vague notion that electrical combinations, when not exhibiting their electrical tension, were in a magnetic state, and that there was a kind of electro-magnetic meridian depending upon the electricity of the earth.—(Annales de Chimie, tom. lxiv. p. 80.) Since this letter has been written, Dr. Marcet has been so good as to send me from Genoa some pages of Aldini on Galvanism, and of Izarn's Manual of Galvanism, published at Paris more than 16 years ago. M. Mojou, sen. of Genoa, is quoted in these pages as having rendered a steel needle magnetic by placing it in a voltaic circuit for a great length of time. This, however, seems to have been dependent merely upon its place in the magnetic meridian, or upon an accidental curvature of it; but M. Romagnesia, of Trent, is stated to have discovered that the pile of Volta caused a declination of the needle: the details are not given; but if the general statement be correct, the author could not have observed the same fact as M. Oersted, but merely supposed, that the needle had its magnetic poles altered after being placed in the voltaic circuit as a part of the electrical combination."

(*To be continued.*)

Fig. 1.

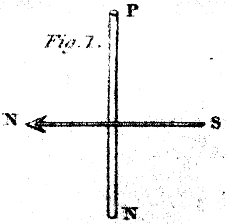


Fig. 2.

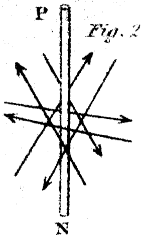


Fig. 3

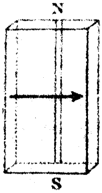


Fig. 4.

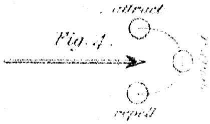


Fig. 5.

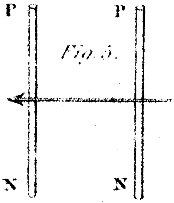


Fig. 6.

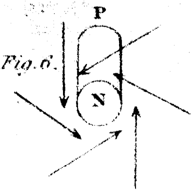


Fig. 7.

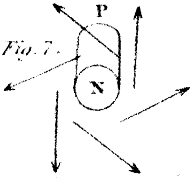


Fig. 10.

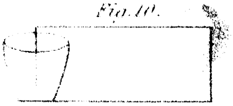


Fig. 12.

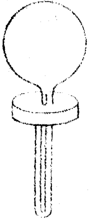


Fig. 8.

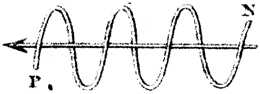


Fig. 11.

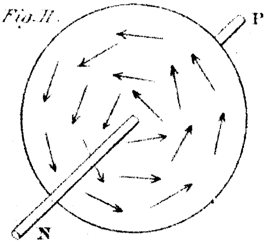


Fig. 9.

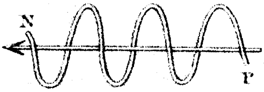
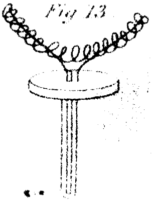
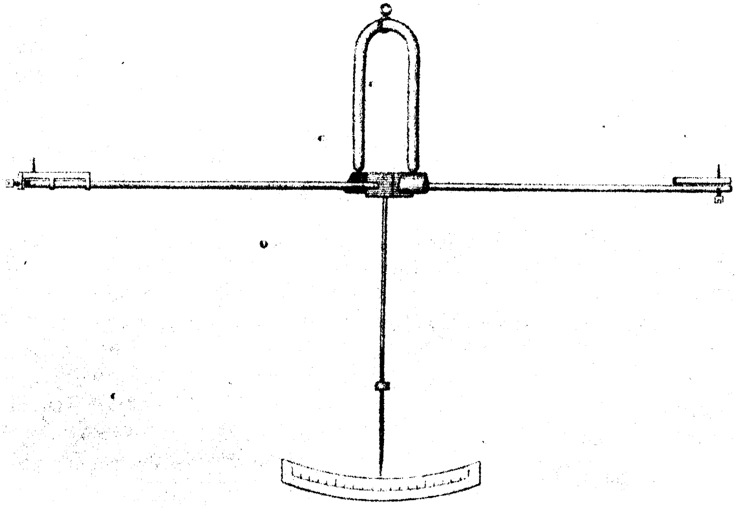


Fig. 13.





Mr W. Herapath's New Balance. Page 291

Engraved for the Annals of Philosophy. Published by Baldwin, Cradock & Joy, [illegible]

ARTICLE V.

Description of a New Balance. (With a Plate.) By W. Herapath, Esq.

(To the Editor of the *Annals of Philosophy.*)

SIR, *Bristol, Aug.* 14, 1821.

A BALANCE without friction, or with as little as possible, has always been a desideratum among chemists. I have, in a great measure, accomplished this in making one for my own use, by suspending the beam from a magnet, as shown in Plate IX. The scales are hung upon the points of needles rivetted into moveable pieces, which are recommended by Mr. Daniell in the last Journal of Science. I have used it with 165 grs. in each scale, and have no doubt but it would have carried 250; it possesses this advantage, that the friction of the axis is reduced as the weight is increased, and, of course, it is most sensible with the greatest weight. The extremities of the magnet are convex to reduce the points of contact; the scales are made of jeweller's foil, and, with the beam, weigh 62·75 grs. When loaded with five grs. in each scale, 1-100th grain moves the index through an arc of 10°. The axis has knife edges, but I intend making one with points, and with two brass rings surrounding it, to prevent its falling in case of accident.

I mean to have a set of magnets of various strengths so as to apportion the attraction to the weight, it will then be most sensible with that weight I wish to use.

I am, Sir, your most obedient servant,
WILLIAM HERAPATH.

ARTICLE VI.

On Mr. Smithson's Hypothesis of the Formation of Capillary Copper. By Charles Konig, Esq.

(To the Editor of the *Annals of Philosophy.*)

DEAR SIR, *British Museum, Aug.* 22, 1821.

MR. SMITHSON's hypothesis, that the capillary copper found in the cavities of copper slags is produced by propulsion of the fused metal through the pores of the surrounding mass, is not less ingenious, than the experiments by which he endeavoured to verify it appear conclusive. The passage of melted tin through cast iron acted upon by intense heat is a fact which considerably adds to the probability, that not only a great portion of capillary native copper, but also silver (especially that from Peru, in the hollows of porous iron-shot quartz) is produced by the same agency, and under similar circumstances. It may not, therefore, be uninteresting to Mr. Smithson to learn, that the above remarkable fact, communicated by him in the *Annals of Philosophy* for April, on the authority of M. Ampère, is already mentioned by Sir James Hall, in his account of the experiments on the effect of heat modified by compression. A substance like wool was formed in several of those experiments by the exudation of the fusible metal through the barrels of iron employed by him; the metal in a liquid state spouting to a considerable distance. There is some of this metallic wool among the specimens, showing the result of his experiments, and deposited by Sir James Hall in the British Museum.

Believe me, dear Sir, very truly yours,

CHARLES KONIG.

ARTICLE VII.

On Wollaston's Thermometer, and its Application in Measuring Heights. By James Apjohn, AB. MD.

(To the Editor of the *Annals of Philosophy.*)

SIR, *Trinity College, Dublin, Aug.* 18, 1821.

THOUGH the Rev. Dr. Wollaston, to whom the credit of the invention of the thermometrical barometer is undoubtedly due, has transmitted to the Royal Society two papers describing its construction, and some measurements made with it, still he does not appear to me to have sufficiently explained its principle, or to have duly appreciated its importance. A few remarks on these topics may, therefore, prove not unacceptable to those who have not hitherto directed their attention to this subject. It is a curious and well-known fact, that the boiling points of liquids are lowered under diminished pressure; and it has frequently occurred to philosophers, that this discovery might be turned to account for the purposes of levelling. All that would appear necessary for putting this method into practice would be the possession of a very delicate thermometer, and a knowledge of the law which indicates the relation between a given fall or rise in the boiling point of water (for example), and the corresponding ascent or descent. Now if from the observation of the boiling points at any two stations, we can infer the pressures due to those temperatures, it is clear that the problem is solved, and that the subsequent calculation is to be conducted as if the densities of the atmosphere were determined by a common barometer. But that we may fully understand the manner in which the pressure, at any station, is inferred from the temperature of water boiling there, or more properly from the temperature of its steams, it is necessary to recollect, that when water boils, the tension of its steam is in equilibrio with the pressure of the atmosphere, and, therefore, that both are susceptible of the same measure. But Dr. Ure has shown, by a number of accurate and well-arranged experiments, that the temperature at which water boils, and the tension of its steam, are so connected that 30 inches of mercury, being its tension at 212° Fahr. $\frac{30}{1\cdot23}$ will be its tension at 202, $\frac{30}{1\cdot23 \times 1\cdot24}$ will be its tension at 192, and so on, the second figure of the decimal part of the ratio increasing by unity for every 10 degrees as we descend, but decreasing by unity for every 10 degrees as we ascend. Thus the tension of steam at 222 is 30 × 1·23; at 232 is 30 × 1·23 × 1·22, &c. That this formula can only be true within certain limits is obvious from the consideration that at all temperatures above 442, we should have the elastic force of steam decreasing while its temperature increased—a conclusion manifestly absurd. However, with such high temperatures, we have at present nothing to do, nor indeed was it at all intended that the formula should extend to them; and for all temperatures about 212°, with which alone we are concerned, I think, from the well-known accuracy of the experimenter, it may be safely relied on. By means of this formula, Dr. Wollaston has constructed a table, the small fragment of which here given, is quite sufficient to enable us to calculate all heights within its range from observations made with the thermometrical barometer.

Boiling points of water.	Corresponding heights of barometer.	Height in feet corresponding to a fall of a degree in the boiling point.
214	31·2395	
213	30·6149	526·320
212	30·0000	528·666
211	29·3948	531·006
210	28·7993	533·352
209	28·2133	535·692
208	27·6367	538·028
207	27·0695	540·378
206	26·5115	543·724
205	25·9627	545·064
204	25·4230	547·404
203	24·8923	549·750
202	24·3704	552·090

The above table might have been more easily constructed by assuming that the tension of steam observes a geometric, while its temperature increases in an arithmetic ratio, nor would it, thus calculated, have been materially less accurate. To illustrate its application, we will here give the particulars of a measurement lately made with an instrument belonging to Dr. Stokes. This instrument was made by Mr. Lovez, of Edinburgh. Its bulb is 1¼ inch in diameter, and it possesses a scale of 13-16ths of an inch to a degree. The degree is divided into 20 equal parts on an attached brass scale, and these are further divisible into 1-20ths by means of a sliding vernier. To this short description, I will merely add, that the point 212 was fixed for a pressure of 30, referring for a more detailed account of the mode of constructing similar instruments to those papers of Dr. Wollaston's already alluded to. Near the gate leading to Dr. Stokes's house at Ballantyne, the instrument, in the steam of boiling water, stood steadily at 211 + $\frac{17}{20}$, the thermometer at the same time indicating a temperature of 68·5; and on the top of the Three Rock Mountain, we found that the boiling point had descended to 209 + $\frac{13}{20}$, the thermometer having likewise fallen to 62. By a reference to the table, then, it will appear that the entire ascent consists of $\frac{7}{20}$ths of 535·692 together with 533·352 and $\frac{17}{20}$ths of 531·006 feet, making a sum total of 1170·199 feet. The correction for temperature is applied in the following manner: Let the tabular height 1170·199 be multiplied by 33·2 (the excess of the arithmetic mean of the temperatures of the two stations above 32), and let the product be multiplied by 208 (which we will call the constant multiplier), and the decimal point be moved five places to the left, the resulting number 80·809, added to the tabular weight, will give 1251·008 feet for the true height of the Three Rock Mountain over Ballyntyne. It will be here observed that I do not use the same correction with Dr. Wollaston. His is greater, and was adopted on the authority of General Roy. Mine is taken from a work on Mechanics, published by the Rev. Mr. Robinson, late Fellow of Dublin College, and is that which occurs in the barometric formula of Laplace.

As has been already stated, the instrument on the Three Rock Mountain stood at 209 + $\frac{13}{20}$, therm. 62; but in about half an hour after, on Garrycastle, an adjacent mountain, it fell to 209 + $\frac{3}{20}$, therm. 62 still. It is manifest, then, that the tabular height is half of 535·692, which, when corrected in the manner already specified, gives for the height of Garrycastle over the Three Rock Mountain 284·559 feet. By adding this to 1251·000 feet, the true height of the Three Rock Mountain over Ballyntyne, we shall have for the elevation of Garrycastle over Ballyntyne 1535·561 feet. Now the heights of these mountains, as determined barometrically by Dr. Fitton and Mr. Weaver (see vol. v. of the Geolog. Trans. Memoir by Thomas Weaver, Esq.) are: Three Rock, 1247·9; Garrycastle, 1531·7, which numbers scarcely differ from those given above. The above results are very satisfactory, and indeed their close correspondence with the barometrical determinations are rather surprising, the deviations being much within the limits of the possible errors of observation.

On the whole, this instrument appears to me much more eligible than the common barometer. It has many advantages over it. I will enumerate some of them. In the first place, it is much more portable—a circumstance of considerable consequence, and one which will be duly appreciated by those who have had sufficient scientific zeal to carry one of Troughton's barometers to the summit of a high mountain. Secondly, it is capable of being made much more sensible. The sensibility of the instrument with which the above observations were made is to that of the barometer nearly in the ratio of 65 to 48. Thirdly, the calculation is much more simple, as is obvious from the examples already given. The table precludes the necessity of having recourse to logarithms; and it is an advantage peculiar to this method, that it is encumbered but with one of the two corrections necessarily embraced in barometric formulæ. Fourthly, an observation is made with surprising despatch; for as the instrument should be always kept in the steam, on account of the superior equability of its temperature, a very small quantity of water in the boiler will be quite sufficient for every purpose.

Dr. Wollaston does not appear to have used his instrument unless in subservience to the barometer. He merely divides an attached scale into a number of equal parts, and knowing the number of these corresponding to a degree of Fahrenheit, and the altitudes of the barometer at the times of the two observations, he is enabled to calculate the true height. This attention to the height of the barometer by a second observer is generally found very inconvenient, and is, I believe, quite unnecessary, unless when the weather is very unsettled, or the interval of time between the observations considerable; or, lastly, where extreme precision is desirable. To render the thermometrical barometer an independent instrument, and even superior in consequence of its greater sensibility to any single barometer, it is only necessary that the point 212 be taken for a pressure of 30, which may be done, under any given pressure, in the following manner: Take the difference between the height of the barometer at the time and the standard height 30; and as 3 is to 5, so will this difference be to the distance (measured in degrees of the scale) above or below the point at which the instrument then stands, at which 212 is to be placed. This fourth proportional is to be measured upwards if the barometer stands below 30, downwards if above 30. The practice of this method evidently presupposes the knowledge of the length of a degree in the scale of the instrument, and this should be determined (as Dr. Wollaston has judiciously remarked) at a low temperature before the instrument is sealed, by comparison with a good thermometer.

Two other measurements have since been made with the therm. barometer which differ very widely indeed from the barometrical determinations. The one was of the greater Sugar Loaf, the other of Douse Mountain. We will give the particulars. On Aug. 12, within a quarter to 11 o'clock, a. m. the instrument stood near Col. Wingfield's lodge at $212 + \frac{1\frac{1}{2}}{20}$, thermometer 62, and same day on the summit of the greater Sugar Loaf within 20 minutes of two, p. m. it stood at $209 + \frac{2\frac{1}{4}}{20}$, therm. 60; and same day, also, on the top of Douse, 31 minutes past four, it stood at $207 + \frac{14\frac{1}{2}}{20}$, thermometer 54. From these data, it follows that the tabular heights of these mountains respectively are 1579·446 and 2326·33 feet. To these numbers a correction must be applied which has not been hitherto adverted to. In the interval between the first and the succeeding observations, a barometer at Dr. Stoke's house, Harcourt-street, rose from 30 to 30·05, where it continued for the remainder of the day. Now as this increment of pressure must have prevented the due fall of the boiling point on the Sugar Loaf and Douse, a compensating correction must be applied to the tabular heights given above. This is easily done. For as ·6 of an inch on the barometer corresponds very nearly to the 212th degree; that is, to 330·006 feet (see table) ·05 of an inch will give 1-12th of a degree, which is equivalent to 44·25 feet. By adding this to 1579·446 and 2326·33 feet, we shall have 1623·696 and 2370·58 feet for the true tabular heights of the Sugar Loaf and Douse. When, to these numbers corrected for temperature, 15·5 feet be added (the height of the lowest station over low water mark), we shall get for the true heights of the Sugar Loaf and Douse 1737·137 and 2514·28 feet respectively. The height of the Sugar Loaf as determined by Dr. Taylor and Mr. Weaver, according to the barometrical method, is 2004 feet, and that of Douse, as similarly determined by Mr. Griffith and Mr. Weaver, is 2392 feet. The elevation, then, we give Douse, is about 122 more than that assigned it by these gentlemen, and that we give the Sugar Loaf about 267 less. It is difficult to reconcile discordancies of such magnitude. It is remarkable, however, that we make the excess of the height of Douse over that of the Sugar Loaf nearly twice as great as that deduced from the barometrical results. Now an error of this amount, I am confident, could not have occurred in our observations. I am, therefore, I confess, at present disposed to account for the want of correspondence between our measurements by some error in the barometric determinations. It is proper to observe here, that all the above observations were made conjointly by me and my friend Mr. W. Stokes.

ARTICLE VIII.

A Series of Observations on the Thermometer, made on June 15, 1821, at Crumpsall, in Lancashire, for the Purpose of ascertaining the most convenient Method of obtaining the Mean Temperature of the Natural Day. By Mr. John Blackwall.

(To the Editor of the *Annals of Philosophy*.)

SIR, *Crumpsall, July* 20, 1821.

SHOULD you consider the following observations and remarks at all calculated to interest your meteorological readers, they are much at your service. Your obedient servant,

JOHN BLACKWALL.

Hour.	Ther.	Hour.	Ther.	Hour.	Ther.
12h night	48·5°	8h 30′	54·2°	5h 0′	65·0°
12 30′	48·0	9 0	55·0	5 30	63·2
1 0	48·0	9 30	56·5	6 0	63·0
1 30	47·0	10 0	57·2	6 30	61·0
2 0	46·0	10 30	59·5	7 0	61·0
2 30	46·0	11 0	61·0	7 30	59·5
3 0	46·5	11 30	63·0	8 0	59·0
3 30	47·5	12 noon	64·2	8 30	57·5
4 0	47·7	12 30	64·5	9 0	57·5
4 30	48·0	1 0	64·5	9 30	56·5
5 0	48·2	1 30	64·5	10 0	56·0
5 30	48·5	2 0	65·7	10 30	55·5
6 0	49·5	2 30	64·5	11 0	55·0
6 30	50·0	3 0	67·0	11 30	54·0
7 0	52·0	3 30	66·0	12 night	54·0
7 30	53·2	4 0	64·5		
8 0	53·7	4 30	64·5		

It has long been a subject of regret among meteorologists, that a greater degree of uniformity is not observed in the time and manner of taking their observations; the irregularity which prevails rendering a comparison of registers kept by different persons almost impracticable, and nearly useless.

It is hardly to be expected, however, that observers will be induced to adopt a more regular system, or that they will be influenced any otherwise than by convenience, or caprice, until the best mode of conducting meteorological observations has been found by direct experiment.

In order to attempt the attainment of this object, as far as regards temperature, on the evening of June 14, a common mercurial thermometer, a Six's thermometer, and a pair of horizontal self-registering thermometers, arranged according to Rutherford's method, were placed in the open air, out of the direct influence of the sun, about a foot from the window of a room on the second floor, having a northern aspect.

The preceding observations were made with the common mercurial thermometer: the first, at 12 o'clock on the night of the 14th, and one each succeeding half hour, to 12 o'clock on the night of the 15th, making a total of 49 observations.

The state of the weather during the whole of this period was as follows: From

Midnight on the 14th to half-past two, a.m. on the 15th, clear and starlight.

Half-past two, a.m. to half-past three, clear and fine.

Half-past three to four, hazy.

Four to half-past six; dense fog.

Half-past six to eight, foggy.

Eight to nine, hazy.

Nine to eleven, hazy, with clouds and faint sunshine.

Eleven to one, p.m., sunny, with some light clouds.

One, p.m. to half-past one, cloudy, with a little faint sunshine.

Half-past one to half-past two, overcast.

Half-past two to four, cloudy, with gleams of sunshine.

Four to half-past five, cloudy.

Half-past five to half-past seven, cloudy, with gleams of sunshine.

Half-past seven to eleven, cloudy.

Eleven to midnight, moonlight, with a few clouds.

The wind was SW. and light, all the time.

The lowest point to which the mercury descended was 46°; this extreme it reached at two o'clock, a.m. and remained stationary for half an hour; when it began gradually to ascend, and reached 67°, its highest extreme, at three o'clock, p.m. The mean of these extremes is 56·50°, and the mean of the 49 observations is 56·37°.

The means of two observations, one taken in the morning, and the other in the evening, that approach the nearest to the mean of the natural day, appear to be those obtained from observations made at

Half-past seven, a.m. and half-past seven, p.m. the mean of which is 56·35°.

Eight, a.m. and eight, p.m. the mean of which is 56·35°.

Nine, a.m. and nine, p.m. the mean of which is 56·25°.

Half-past nine, a.m. and ten, p.m. the mean of which is 56·25°.

Ten, a.m. and half-past ten, p.m. the mean of which is 56·35°.

The nearest means of three observations seem to be those found from observations taken at

Half-past seven and half-past ten, a.m. and half-past nine, p.m. mean 56·40°.

Eight and half-past ten, a.m. and ten, p.m.; mean 56·40°.

Half-past eight and half-past ten, a. m. and half-past ten, p.m.; mean 56·40°.

Half-past eight and eleven, a. m. and half-past eleven, or twelve, p. m.; mean 56·40°.

Half-past seven, a. m. and half-past six or seven, and eleven, p. m.; mean 56·40°.

Half-past seven, a. m. and half-past seven and half-past nine, p. m.; mean 56·40°.

Eight, a. m. and half-past seven and ten, p. m.; mean 56·40°.

Half-past eight, a. m. and half-past six or seven, and half-past eleven or twelve, p. m.; mean 56·40°.

Half-past eight, a. m. and half-past seven and half-past ten, p. m.; mean 56·40°.

Half-past eight, a. m. and eight and ten, p. m.; mean 56·40°.

Ten, a. m. and half-past nine and half-past ten, p. m.; mean 56·40°.

The extremes by Six's thermometer were 46·3° and 65°, the mean of which is 55·65°; and the extremes by Rutherford's thermometers were 47° and 66·5°, the mean of which is 56·75°.

It is very probable that the mean temperature of the natural day is generally taken too high in summer; especially when found from three observations, one of which is made at one o'clock, p. m. The most convenient method of taking it, and, perhaps, as accurate a one as can be adopted, having the additional advantage also of giving the extremes of temperature for the period, which are not obtained by the usual modes of observing, is to find the mean of the daily extremes registered by a good Six's thermometer, or by a pair of Rutherford's horizontal thermometers. This result will not be far from the truth. A preference, however, will generally be given to Rutherford's thermometers, as they are rectified by simply raising one extremity of the frame to which they are attached, and letting the indexes slide, one to the end of the spirit, and the other to the end of the mercury; whereas the application of a magnet is necessary to bring the indexes of a Six's thermometer into their proper situations.

For the more perfect attainment of the object under consideration, another series of observations similar to the foregoing should be made in the month of December.

Article IX.

Astronomical Observations, Sept. 1821.
By Col. Beaufoy, FRS.

Bushey Heath, near Stanmore.

Latitude 51° 37′ 44·3″ North. Longitude West in time 1′ 20·93″.

Sept. 11. Ingress of Jupiter's first satellite	10h	23′	29″	Mean Time at Bushey.
	10	24	50	Mean Time at Greenwich.
Sept. 11. Emersion of Jupiter's third satellite	10	30	12	Mean Time at Bushey.
	10	31	33	Mean Time at Greenwich.
Sept. 12. Immersion of Jupiter's second satellite	13	29	[illegible]	Mean Time at Bushey
	13	30	40	Mean Time at Greenwich.
Sept. 18. Emersion of Jupiter's third satellite	14	30	09	Mean Time at Bushey.
	1	31	30	Mean Time at Greenwich.

Article X.

Analysis of Tabular Spar from Pargas. By P. A. V. Bonsdorff, Ph. D. of Åbo.*

Among many rare minerals which occur in the lime quarries of the parish of Pargas, there is a white radiated substance, which was for a long time considered to be tremolite; but an analysis which I have performed shows that this mineral is tabular spar. It occurs principally in the lime quarry of Skräbböle, in the parish abovementioned; it is accompanied with granular calcareous spar, blackish sphene, and an amorphous mineral of a reddish colour, resembling idocrase, or garnet.

The colour of this tabular spar is white, translucent at the edges; its lustre vitreous, but not very considerable; it is scarcely hard enough to scratch glass, and its fragments are filiform. By the blowpipe, at a high temperature, it melts at the edges into a translucent shining glass; with borax and microsmic salt, it forms a clear glass; with soda, the glass is opaque; when heated with solution of nitrate of cobalt, it assumes a blue colour, proving that it does not contain magnesia, and, consequently, that it is not tremolite.

I had great difficulty in procuring pure fragments of this mineral for analysis; because it is mixed with calcareous spar, with very small grains, of a green substance, resembling actinote, and of a white one, which seemed to be quartz. After these had been separated, as much as possible, by mechanical means, I endeavoured to dissolve the remaining portions of calcareous spar by acetic acid; but I found that this mineral possessed the unusual property of being to a considerable extent decomposed by acetic acid. I had, therefore, no other method of separating the calcareous spar, than that of treating the levigated mineral with an aqueous solution of carbonic acid, and this answered the purpose extremely well.

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* Extracted from Memoirs presented to the Academy of Sciences in Petersburgh.

I now proceeded with the analysis as follows: 175·5* parts of the purified powder of tabular spar were mixed with three times their weight of carbonate of potash, and heated in a platina crucible; the mass treated with muriatic acid, the solution evaporated to dryness, and the residuum again treated with muriatic acid, left the silica, which when washed, dried, and ignited, weighed 96·2 parts. The muriatic solution saturated with ammonia afforded a small quantity of precipitate, which, when boiled with solution of potash, exhibited only a trace of alumina; the portion insoluble in potash consisted entirely of peroxide of iron, which weighed, after ignition, 2·3 parts = 20 of protoxide of iron.

Carbonate of ammonia added to the solution from which the alumina and oxide of iron had been separated by ammonia, gave a white precipitate, which was carbonate of lime; it weighed 138 parts = 77·8 of lime. This carbonate of lime was dissolved in a mixture of muriatic and sulphuric acid, and evaporated to dryness. The ignited sulphate of lime weighed 185·4 parts = 77 of lime; there is, consequently, a little difference in the weight of the lime deduced from the carbonate and the sulphate. I shall, therefore, take the mean = 77·4 as the true quantity of lime: a little water was added to the sulphate of lime, and the solution obtained being examined, was not found to contain any magnesia.

The solution which remained after precipitation with carbonate of ammonia was heated to ebullition, and solution of carbonate of potash added to it. This gave a precipitate which, when washed and ignited weighed 2·6 parts. This treated with sulphuric acid evaporated to dryness, and treated with a small quantity of water, left 3·3 of sulphate of lime = 1·4 of lime, which, deducted from 2·6, leave 1·2 as the quantity of magnesia contained in the mineral.

In order to determine the quantity of volatile matter contained in this mineral, a portion of it that had been purified, and which weighed 50·5 parts, was ignited for an hour in a platina crucible; it then weighed 50·0, again exposed to heat increased by the bellows, it suffered no further diminution of weight. According to this experiment, the loss by heat amounts to 0·99 per cent.

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* The quantity actually employed was 1·755 gramme.

The analysis, consequently, gives as the constituents of the tabular spar:

Silica	52·58	containing 26·45 of oxygen
Lime	44·45	12·49
Magnesia	0·68	0·26
Protoxide of iron	1·13	0·26
Alumina	Trace	
Volatile matter	0·99	
	99·83	

It is to be observed that the small portions of magnesia and oxide of iron can only be considered as mechanically mixed, but even in this state they must be combined with a portion of silica, and very probably they form a bisiliciate; then 0·26 + 0·26 = 0·52 and 0·52 × 2 = 1·04; consequently, this quantity of oxygen, when subtracted from the oxygen of the silica, will leave it 25·41; and when we double the oxygen of the lime, we shall have 24·98, which is very nearly equal to the quantity of oxygen in the silica; and it may be further observed, that the quantity of silica is probably increased by the imperfect separation of the small grains of quartz: this mineral is, consequently, a bisiliciate of lime, and its mineralogical formula as $C\ S^2$.

It is stated by Klaproth, who first analyzed the tabular spar, that this mineral from Dognatzka, in the Bannat, besides 50 per cent. of silica, contained 5 per cent. of water. On this account, I have endeavoured, as much as possible, to ascertain the true quantity of volatile matter; but the results of our experiments indicate great difference in this respect. It is, however, known, from the examination which M. Berzelius has made of this mineral from the Bannat with the blowpipe, that it contains no water at all, and, consequently, the difference of composition which appears to exist in the minerals is explained.

As it is proved that the tremolite from Pargas agrees perfectly in composition with the tabular spar, it may be concluded that the latter has been frequently mistaken for the former. At the village of Perhonjemi, in the government of Kymmene, in Finland, there occurs a fine white radiated mineral, which has been considered to be tremolite, but a recent analysis of it by M. Rose gives 51·60 silica and 46·41 lime, with 1·11 of actinote mechanically intermixed. At Gökum, near Danemora, in Sweden, a mineral is found, which very much resembles the tabular spar from Pargas, and which has been found to be similar also in composition.

ARTICLE XI.

Reply to X.'s Remarks on Mr. Herapath's Theory.

(To the Editor of the *Annals of Philosophy.*)

DEAR SIR, *Cranford, Sept.* 1, 1821.

THE object I have all along professed to have in view is the investigation of truth, and with this object I shall ever be ready to attend to any remarks on my writings made in an open and a gentlemanly manner, when they display in their author a sufficient knowledge of the subject to entitle him to attention. With respect to your correspondent X. he has at least introduced and closed his "Remarks" in a way that cannot fail of eliciting my approbation. Having said this much, I trust he will not be offended at my requesting him to read *all* that I have published on the subject, to re-peruse the papers he has attacked, and to reconsider the one he has written. Probably your correspondent will likewise allow me to observe, that the addition of his name would not induce me to pay less attention to his remarks; while it would screen them from that jealousy and suspicion of motive with which, I believe, anonymous attacks are now usually regarded, and which circumstances, connected with the fate of the latter paper, are by no means calculated to remove.

I am, dear Sir, yours faithfully,

J. HERAPATH.

ARTICLE XII.

On Mr. Tredgold's "Refutation" of Mr. Herapath's Theory.

(To the Editor of the *Annals of Philosophy.*)

DEAR SIR, *Cranford, Sept.* 20, 1821.

I THANK you for directing my attention to Mr. Tredgold's "Refutation of my Theory," in the Philosophical Magazine, which I had not had the pleasure of reading. By this gentleman's observations, I cannot help thinking, that, like your correspondent X, he has taken up his ideas of my paper a little too hastily. It is much to be feared whether the letter that stands at the head of my first paper, indicating some dissatisfaction on my part with certain members of the present Council of the Royal Society, may not have induced many individuals to conceive that these members easily saw through the defects of my views; and, therefore, treated the paper with that contempt which is the merited fate of visionary pretensions. I will not say that such is the opinion of Mr. T. and X.; but I much question whether that letter has not led them to form a less favourable opinion of the subject than they otherwise would, and consequently to imagine it no difficult matter to refute a theory which, they suppose, had been already condemned by our highest scientific tribunal. Whether I miscontrued the conduct of those members of the Council, and thus acted from mistaken feelings; or whether that conduct was not such as I had a right to expect, is a question I am not here disposed to discuss. Suffice it to say, that repeated and mature considerations of the whole correspondence and concomitant circumstances have not, in the minds of some respectable and competent judges, created one idea unfavourable to myself as a man, or as a philosopher; and were Mr. T. acquainted with the whole of the case, he would not, I am persuaded, see any impropriety in my repeating to him the request I have made to X. "re-peruse the papers you have attacked, and reconsider your own." However, lest any one should imagine I employ this as a subterfuge to avoid a discussion the consequences of which I have reason to apprehend, I beg leave, in justice to my own character, to cite a few passages from the letters of Sir H. Davy, the President, and D. Gilbert, Esq. the Vice-President, which will serve to show the weight of the objections that a ten months' consideration enabled some of the ablest members of the Society to make, as well as the opinion they entertained of the communication and its author.

The first I shall quote is from a letter of Mr. Gilbert, dated June 6, 1820.

"DEAR SIR,

"I had sometime since the pleasure of receiving your very curious investigation on the cause of gravity. I read it over; and although I must confess myself not satisfied with the ultimate deductions, yet I was much pleased with the great ingenuity displayed throughout the whole; but I entertained my doubts on the propriety of laying before the Royal Society any thing so abstruse and metaphysical. I, therefore, desired two of the *best* mathematicians in London *to look at the premises*, and their opinions have confirmed my doubts. They say such a work should be laid before the public in a separate form."

In another letter, dated Oct. 25, 1820, this gentleman says:

"You would of course wish to avoid the paper's being read before the Society, and then not ordered for printing by the Council. I, therefore, endeavoured to ascertain the opinion of some members of the Council, who are usually looked up to on such occasions, and they considered the investigations as too theoretical for the Transactions *without taking on themselves to judge of the mathematics.*"

In a letter from Sir H. Davy, dated Jan. 13, 1821, he says:

"I have read the parts of your paper, which are intelligible *without profound mathematical study*, with attention; and highly ingenious as I find your views, I must say I am not impressed with a conviction of their truth.

"The pressure of your gravific fluid, for instance, taking away weight, must depend upon the *motion* of its particles; and yet you counteract this pressure by *heat* which you consider as motion."

On this passage I beg leave to observe, that a paper such as this, which is strictly mathematical, cannot be judged by general views. The President himself was aware of this; and in a conversation I had the honour to have with him at one of his Wednesday evening meetings, intimated that he laid no stress at all on his objection.

In the same letter, Sir H. says,

"There is *so much ingenuity, and so minute an acquaintance with the progress of discovery*, displayed in your paper, that I cannot help wishing that its views and objects had been directed to a matter of pure experimental inquiry. For instance, the doctrine of heat and the investigation of its laws, supposing it to be motion. *Such a preliminary paper, if satisfactory, would prepare the philosophical world for greater and more abstruse researches.*"

In a note of Jan. 30, Mr. Gilbert, speaking of the reason of his having first objected to present the paper to the Royal Society, says,

"On your account, as well as on my own, I did not like to present a paper of *so much importance, and embracing so very wide a field*, without consulting two or three members," &c.

The only other passage I shall quote is from a note of Sir H. Davy, dated March 6.

"Having considered a good deal the subject of the supposed real zero," says Sir H. Davy, "I have never been satisfied with any conclusions respecting it. I cannot see any necessary connexion between the capacity of bodies for heat and the absolute quantity they contain; and temperature does not measure *a quantity*, but merely a property of heat."

In my reply to this part, dated March 8, I observed:

"You are aware that I conceive heat to consist in motion; and that the temperature of a body is the intensity of the intestine motion of its particles estimated, when you compare the temperatures of different bodies, not by their velocity, but their momentum. The degree, therefore, of absolute cold is where the particles have no motion; and my object has been to ascertain this by determining the ratio of the intestine motions at two fixed points, as those of ice melting, and water boiling. What is called the "capacity of bodies for caloric," I have demonstrated in my memoir to be owing to the numeratom of the bodies; that is, the number of particles which there is in a unity of their volume."

These extracts are taken out of near 40 letters which have passed on this subject; and except an acknowledged erroneous mathematical charge made by a gentleman, whom for obvious reasons, I do not mention, contain all the objections hitherto raised. I hope, therefore, they will satisfy the world that difficult as the subject is, I have not, in the opinions of those who are conceived to be our most scientific men, treated the matter in a manner very easily to be refuted. In fact, the whole difference between me and certain members of the Council has not been on account of any defined or undefined charge of error—

Under these circumstances, I trust that I may be allowed, purely from a wish to avoid precipitate controversy, to recommend not only Mr. Tredgold, and your correspondent X. to re-examine their difficulties and objections, but likewise any other individuals who may be disposed to honour my views with their notice.

At present I perceive both Mr. T. and X. have fallen into errors and misconceptions; and one, I regret to say, into misrepresentations, which I would rather see corrected by himself than by me.

Though I have no intention now to enter into the subject, I cannot help noticing the new and peculiar manner in which Mr. Tredgold has managed his "Refutation." Without advancing one fact against my views, he propounds a new theory of collision, partly borrowed from mine, and partly the offspring of his own imagination, but agreeing neither with the old theory, nor with mine, nor with that of any other individual; and then he tells us, not indeed in words, but in substance, that 'his conclusions being most of them different from Mr. Herapath's, his (Mr. T.'s) Refutation is complete, and Mr. H.'s theory falls to the ground.'

"What kind of a theory," a friend asked me the other day, "could you write to satisfy both Mr. T. and X.? Would it not be advisable," said he, "for these gentlemen to settle their own discordances before they venture to attack another?"—(See their opinions of aëriform elasticity at the bottom of p. 131, Phil. Mag. for Aug. and bottom of p. 224, *Annals* for Sept.)

I now beg leave to inform your readers, that the paper attacked has but one first principle, that of absolute hardness in the ultimate atoms. The postulata, though they are put in this form, are not, as I have in that paper hinted, incapable of being established by rigorous demonstrations; but I chose this way because, except the third, they are all Newton's maturest ideas;*

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* It has been usual to cite Newton as having demonstrated the repulsive force of gaseous particles, but the following observations will show, that he did not at all consider it as proved. "An vèro Fluida Elastica ex particulis se mutuò fugantibus constent,

and because by this course I should myself, as I have stated, avoid being obliged to attack the works and refute the notions of many respectable individuals. From my first principle, I have proceeded entirely by what appears to me to be strict mathematical induction. The communication is, therefore, to be considered mathematical, and can hence be refuted only by showing that the principles are false, among which, to give every advantage to its opponents, I will reckon the postulata; by mathematically refuting the inductions, or by showing that the results do not agree numerically with the phænomena they profess to explain. To avoid metaphysical difficulties, the principles may be passed over by the admission of a simple axiom in philosophy; namely, that it is impossible, by correct reasoning from false principles to bring out true conclusions; and hence the attempts at refutation may be confined to the mathematics and the results. To any respectable efforts of this kind, I shall be happy to pay every attention; but I hope the testimonies in the extracts I have given in this letter, and the extent of my researches in that paper, and the one I am now publishing in the *Annals*, will be admitted a sufficient proof that I have not thought lightly on the subject; and, therefore, I trust, the world will not henceforward require that I should reply to every one who chooses to publish his undemonstrated opinions.

I am, dear Sir, yours truly,

J. HERAPATH.

ARTICLE XIII.

On the perpetual Renewal of Leases. By Mr. James Adams.

(To the Editor of the *Annals of Philosophy.*)

SIR, *Stonehouse, near Plymouth, July* 9, 1821.

THE insertion of the following problem and solution in the *Annals of Philosophy*, when convenient, will oblige

Your humble servant,

JAMES ADAMS.

Problem.—A copyhold estate depending upon three of the *best* lives that could be found is granted on condition that the lessee and his successors, whenever a life may drop, shall be

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Quæstio Physica est. Nos proprietatem Fluidorum ex ejusmodi particulis constantium mathematice demonstravimus, ut philosophis ausam præbeamus quæstionem illam tractandi."—(Principia, lib. 2. Schol. Prop. 23.)

allowed to replace it with the *best* they can find; to determine the probable time between each nomination.

Solution.—The present value of 1*l.* payable every x years for ever at e per cent. compound interest is equal to $\frac{1}{(1+e)^x-1}$. (Baily on Interest and Annuities, p. 77.) The present value of all the renewals for ever is equal to $3\left(\frac{1}{e\,A}-1\right)$, where A denotes the value of an annuity of the lives proposed to be added. —(Baily on Life Annuities and Assurances, p. 174.)

Hence we have the following equation, viz. $\frac{1}{(1+e)^x-1} = 3\left(\frac{1}{e\,A}-1\right) = m$, from whence $(1+e)^x = 1+\frac{1}{m}$, and per logarithms $x \times l\,(1+e) = l\,.\left(1+\frac{1}{m}\right) \therefore x = \frac{l\left(1+\frac{1}{m}\right)}{l\,(1+e)}$, a general theorem for the times between each nomination.

Since A, according to the tables given in vol. ii. Baily's Annuities, may have five different values, x, in consequence, will vary accordingly, which values are selected and placed in the following table:

Where the tables are kept.	Number of tables.	Ages.	4 per cent.		5 per cent.	
			Values of A.	Values of X.	Values of A.	Values of X.
France.......	V	9	19·022	18·435	16·213	18·173
Sweden......	XVI	6	18·725	17·605	16·030	17·476
Northampton..	XXVII	8	17·662	15·020	15·226	14·843
London......	XLVII	10	16·400	12·545	14·300	12·456
Demoivre....	XLIX	10	16·882	13·411	14·607	13·186
			Sum....	77·016		76·134
			Mean...	15·403		15·227

Each of these means corresponds nearest to Table XXVII. Hence the probable time between each nomination, according to the above table, is about 15 years.

By the Carlisle Tables, as given in Mr. Milne's Annuities, the value of A at 4 per cent. is 19·792 years, and at 5 per cent. 16·79, the corresponding age being seven years; from whence the value of x, according to the former, is 20·8656 years; and, according to the latter, 20·6852.

ARTICLE XIV.

On the Means of distinguishing between Freshwater and Marine Shells, independently of the Animal Inhabitant. By G. B. Sowerby, FLS.

(To the Editor of the *Annals of Philosophy*.)

SIR,

I HAVE been lately under the necessity of asserting the freshwater nature of many of the shells found in a fossil state in a stratum, which has been described by Mr. Webster (I believe, the only person who has spoken decidedly about it) as *wholly marine.* I am not aware of how much consequence the question may be deemed in a geological point of view; but it does appear to be of some consequence that facts should be viewed as they really exist, and absolutely necessary that they should be so viewed before we can hope to arrive at certainty in our conclusions. I may, therefore, perform an acceptable service to science in general, and to Geological Science in particular, if I endeavour to point out some of the means by which these two Classes of Freshwater and Marine shells may be distinguished from each other. The subject is important; for it is by knowing the nature of the fossil shells contained in a stratum that we are enabled to decide upon the nature of the stratum itself; and it is also difficult: for if we dismiss the animal inhabitant and the Generic Characters of the shells themselves, there will remain scarcely any general character, or circumstance, upon which to decide; but it will be recollected that the student in Geology is *always* under the necessity of deciding without the animal, and, frequently, he must decide without having enough of the Generic Characters. It may be said, that having ascertained the nature of *some* of the shells contained in a stratum, he may conclude by a sort of natural induction, that the remaining species belong to the same Class; but if he attempt to do this, he will soon find himself in error, for I have lately shown that there exists a bed in which shells decidedly freshwater are mixed with others as decidedly marine; and De Ferussac appears to have made a similar observation, if I understand rightly the following passage from a letter received lately from him: "Les échantillons que je connois me font penser qu'il existe une couche de mélange, ou vos Cyclades, les Melanopsides et des Paludines se trouvent avec des Coquilles marines, entr'autres des Cerites et des Huitres."

Some have thought the comparative *thickness* of the shelly matter might serve as a general distinguishing mark. (Webster in Transac. of Geolog. Soc. vol. ii. p. 211, where, speaking of the "Lower Freshwater Formation," he says, "On a careful examination, I was not able to discover any mixture of marine shells in this series of beds. Had they ever existed, I think their remains would have been evident, considering how much thicker and stronger marine shells in general are than those of fresh-water.") This is quite a mistake. Marine shells are not generally thicker in their substance than freshwater shells; it is true that many of the marine shells are very thick, and comparatively much thicker than a great many freshwater shells; but then, on the contrary, a great number of the freshwater shells are also very thick, and comparatively much thicker than many marine shells, and some of the marine shells are decidedly among the most delicate and thin; and we shall find this at best but a variable character, greatly dependent upon accidental circumstances, such as age, disease, situation among rocks, &c. and the same species is sometimes very thin, at others, very thick. Many shells which are thin in their young state increase in thickness by age; disease is also the cause of extraordinary thickness in *some* cases; and Testaceous Mollusca *which* live in the rocky beds of rapid currents are obliged to accommodate themselves to circumstances, and thicken their shell accordingly.

The epidermis, or, at least, the sort of epidermis which generally coats freshwater shells, appears to be a character deserving of some consideration; but though I believe all freshwater shells have an epidermis, yet I do not think it is always the same sort of epidermis: that is, it appears to vary in thickness as the shell it coats varies; the thin and more delicate shells, such as the Limnei, having a thin, nearly transparent epidermis; while the thicker shells, such as the Uniones, have a thick and almost black epidermis. Geologists, however, seldom have it in their power to observe the epidermis in fossils, though this is sometimes the case: an instance is upon record which I must here mention. Brongniart says: "On trouve encore ce Potamide* dans le forét de Montmorency au dessus de St. Leu; il *y* a conservé son tést et la couleur brun roussâtre qui appartient generalement aux coquilles fluviatiles." Now this colour does not

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[*] I cannot avoid noticing the fact, that Mr. Webster has charged Lamarck with having instituted a Genus, founding its distinctions not on any difference in its form, but on the difference of the water in which it had lived. Mr. W.'s own words are to be seen in Geol. Trans. vol. ii. p. 230, second note. The truth, however, is, that the Genus is founded by Brongniart, and not by Lamarck. Brongniart's words are: "Nous proposons d'établie ce Genre qui est fondé plutôt *sur les habitudes des animaux qu'il renferme, que sur l'importance des caractères exterieurs*: il diffère, en effet, tres peu des Cerites; mais on remarquera que dans le Genre des Cerites, établi par Bruguiere, il y en a qui sont habitantes ou des marais voisins de la mer, ou des eaux saumâtres de l'embouchure des fleuves; or celles de ces éspèces que nous avons vues ont les caracteres que nous attribuons au Genre Potamide, aussi avoient elles été toutes placé dans la division du Genre Cerite qui a pour caractere distinctif *un canal droit et très court*."

I might very reasonably insist upon the superior importance of characters drawn from the form and habits of the animal inhabitants of shells; yet it will appear that this Genus is not founded merely upon the supposed fact of its having inhabited freshwater, since we see that it possesses some particular *external marks* of distinction, marks which characterize all those shells found in situations which may from analogy be supposed to resemble that in which this fossil occurs.

belong to river shells when deprived of their epidermis; these specimens consequently must have been covered by it, not that I mean to advance the presence or absence of an epidermis as a distinctive character between freshwater and marine shells; but I have good reason to believe, that this peculiar kind of epidermis is not found on the marine Cerithia,* having never seen any on such as have hitherto come under my observation. This epidermis appears to be quite necessary to the Potamidæ, and indeed to freshwater shells in general, to enable them to resist the action of the water; for we find that wherever, by any accident, or circumstance, this epidermis is worn off, or otherwise gone, the shells are eroded, sometimes very deeply; and this happens to those parts of the shells which have been for the longest space of time exposed to the action of the water, such as the *umbones* of bivalves, and the point of the spire in turreted shells; and, which may appear singular, the water of the sea does not seem to possess the same power of corroding those parts of shells which, having lost their epidermis, are thus exposed to its action—if I may judge from a specimen of a bivalve shell now before me which has lost its epidermis at its *umbones* and its most prominent parts, and remains otherwise unaltered. It is also to be remarked, that this erosion takes place upon the more prominent parts, and the parts that have been longest exposed to the action of the water, not only in rivers, but in lakes, and that it is common not only in the immense continental lakes, but in some of those of our own island: in fine, it is so common in shells that are certainly of a freshwater nature, that I consider it as one of the best characters by which they may be distinguished from marine; and particularly so, as I do not recollect any instance of such erosion in any shells that are decidedly marine. There are, however, two accidents to which marine shells are subject; the one is, that of being grown over by an irregular crust of coralloïd matter; the other, that of being pierced by immense multitudes of minute vermes; both these accidents produce an appearance somewhat resembling the erosion above spoken of, but may be easily distinguished from it, inasmuch as the one is a mere corrosion similar to that produced by the action of acids; while in the latter cases, the shell is irregularly beset with minute pores, or covered by a very minute *madreporoïd* substance.

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* Although some of those univalves which in the general contour of their shells resemble the species of the decidedly marine Genus Cerithium, are undoubtedly the inhabitants of the mouths of rivers and situations where, the fresh and salt water may become mixed, it is not, therefore, to be supposed, that all are; for there are instances of similar shells from the interior of Africa, and in some of the most rapid rivers far above the point of union between the salt and freshwater. Their presence, therefore, in a stratum, may be considered rather as an evidence of its freshwater than of its marine origin. In the Danube, at Waitzen, in Hungary, a circumstance that will appear still more extraordinary, there is a *bivalve* so exactly similar in its Generic Characters to some of the common grooved Mytili (of Lamarck), that, from recollection, it is impossible for me to mention any one in which it differs. I say I do not recollect any Generic Character in which it differs from decidedly marine species; but I am nevertheless persuaded, that if we knew its animal inhabitant, or had opportunity of studying it in its native situation, we should soon find some discriminating character in the form, as well as habits, of the animal, and probably also in the form of the shell; nor have I the least doubt that this will prove to be the case as well in the Cerithioïd shells as in the bivalves, as we know it to be already in such as we have the means of observing.

It will be obvious, from what has been said above, that the erosion upon the points of univalves, and upon the umbones of bivalves, can never be considered as a distinguishing character of a Genus or Species, seeing that it exists in a greater or less degree in every Species and Genus of freshwater shells. Consequently the terms "*umbones decorticati,*" "*nates decorticati,*" and others which have the same meaning, may always be struck out of the Generic and Specific Characters of shells wherever they occur.

Article XV.

Proceedings of Philosophical Societies.

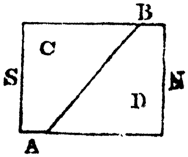
WERNERIAN SOCIETY.

March 10.—The Secretary read some details by Mr. Trevelyan regarding the History of Craniology, from which it appeared, that this science is not so modern in its discovery as has been supposed. The whole data were illustrated by copious extracts from authors of considerable antiquity. Several of these seemed to have been acquainted with the most prominent doctrines advanced on the subject.

Mr. Trevelyan exhibited to the Society a plan of the Rocks at Bamborough Castle, and presented specimens illustrative of their geognosy.

A notice was read from Dr. Knox, who had lately arrived from Africa, regarding a Caffre Albino.

Mr. John Deuchar read an account of three large Loadstones: these were probably the most powerful which have been noticed. The largest carried a weight of 205 lbs. when in Mr. Deuchar's possession; it now belongs to Dr. Hope, the Professor of Chemistry. It is a singular circumstance connected with this magnet, that its south pole is at some distance from the south extremity, and runs in a slanting direction, as represented by A B; the mass C has no sensible magnetic power. This mass, he says, may be common ironstone, ironstone paste, or a distinct loadstone; but he rather supposed the last the most likely; in which event, by removing C, the sum of magnetic power will be increased.



Mr. Deuchar also read his third paper upon the Nature of Flame. In his former papers, he had noticed the fact of the flame occasionally passing through gunpowder without inflaming it; he now traced the cause of that curious result.

March 24.—The Secretary read some Geological Observations made by Mr. Williams on the coast of Labrador, and in Newfoundland.

He also read a paper, by Mr. Anderson, of Inverness, on the Geognosy of the Great Glen of Scotland; and an account by Mr. Edmonston of a new species of Gull.

Mr. Trevelyan read a notice of some remarkable Hail Storms.

A Tartar book, belonging to Lord Hastings, was handed round the Society; while some remarks upon it were read by Mr. Burke.

At this meeting were exhibited the specimen of the Walrus sent by Capt. Parry from Barrow's Strait, which was in excellent state of preparation; also the Tapir of Malacca, and the Junglecock of India.

April 7.—On account of the funeral of the late Dr. Gregory, Professor of the Practice of Physic, the Society adjourned without proceeding to business.

April 14.—The reading of Professor Agardh's paper on the Metamorphosis of Sea Platels was commenced.

A paper on the Geography and Tin Mines of the Island of Banca.

Account of Meteorological Observations made by the Rev. Mr. Macritchie in Perthshire.

Mr. Trevelyan exhibited an Etching of a Fossil Tree, on which he made some observations.

A notice was communicated regarding the Plantings of the Duke of Athol in Perthshire.

Mr. Stevenson read a paper, giving an Account of the Explosion of a High Pressure Steam-Boiler at Lochrin Distillery, near Edinburgh.

April 21.—A Biographical Account of the late Dr. William Wright.

A narrative was read of Dr. Colladon's Descent in a Diving Bell.

The reading of Professor Agardh's paper on the Metamorphosis of Algæ was continued.

A communication from Dr. Fleming was read, giving an Account of a remarkable Species of Trichia, found growing in a Solution of Succinate of Ammonia.

May 19.—A paper was read by Dr. Knox on the Meteorology of the Southern Peninsula of Africa, and on the Temperature of the Northern Atlantic Ocean. From numerous observations, the author ascertained the medium temperature of the southern peninsula of Africa to be about 66° of Fahr. and thinks that this is applicable to that vast tract of country lying between latitudes 28° and 34° S. and longitude 18° and 28° E. The author shows that the climate of the interior of Southern Africa, as it regards moisture, is nearly the reverse of that prevailing in the Cape peninsula; the number of rainy days throughout the year were 76; 15 only of which occurred during the winter months. The general direction of the winds NW, or SE: and the annual table gives 135 days on which northerly winds prevailed; 179 days of southerly winds; 21 westerly; and 11 easterly. The latter winds are often accompanied with a great mortality among horses.

Dr. Knox next demonstrates, that the climate of the Cape is not favourable for those of consumptive habits, though in other respects extremely salubrious.

In the second part of the paper a table is given, showing the temperature of the air and of the ocean between the latitudes of 50° 2′ and 20° 24′ N, and longitude 7° 7′ and 24° 27′ W. The table and annexed observations confirm the remarks of Peron, Humboldt, and others, and establish, beyond all doubt, the remarkable equability of temperature observable in atmospheric air resting over the great ocean. To this quality in the air above, the author ascribes the advantages derived by the phthisical in long voyages; and the total inutility of sailing along a coast, or in the immediate vicinity of any land.

Finally, from observations made on that peculiar climate, called by sailors "The Rains," compared with what he has seen in other countries, Dr. Knox is of opinion, that the doctrines regarding marsh miasmata, and their noxious effects on the human frame, are problematical, and moreover unnecessary in explaining those deleterious effects, which he thinks are ascribable to the combination of heat and moisture only.

The reading of Professor Agardh's paper, on the Metamorphosis of Algæ, was concluded. It is impossible by any abstract, however copious, to do justice to this important paper. The details are minute and interesting; and the facts introduced in support of these are numerous and valuable. It will soon appear in the Transactions of the Society, which we understand it is in contemplation to publish quarterly, or half-yearly.

Mr. Falconer read a notice regarding the Tulipa Oculus Solis, and another beautiful flower, of which drawings were exhibited.

Mr. John Deuchar read a paper, containing Observations on the Occasional Appearance of Water in the Cavities of regularly shaped Crystals, and on the porous Nature of Quartz, and other crystalline Substances, as the probable Cause of this Circumstance. In this paper he supposes that natural and artificial crystals have a similar origin, and that although the former possess a peculiar compactness of cohesion and tardiness of solution, which do not belong to the latter, yet that the artificial ones, through length of time, would acquire a similar cohesion and insolubility. Mr. Deuchar holds, that the water of crystallization in artificial crystals is in great excess, and that this excess, under certain circumstances, gradually decreases. He holds, that the water escapes by capillary movement through the pores of the salt till what he supposed to be the neutral state be acquired, when the salt becomes compact and insoluble. When any malformation of the nucleus produces a cavity or partial vacuum in the interior of a crystal, then the capillary attraction may be exerted to that cavity as well as to the surface; but that this is modified by various accidental circumstances. By pouring hot water upon a crack in the mouth of a bottle, about three inches in length, it extended to five inches, but returned again when he stopped adding the hot water; cracks in pieces of window glass were, also, extended by pressure, and contracted again upon its removal: hence Mr. Deuchar concludes, "that water may enter the void interstices of crystals, when aided by pressure, not only from the porous nature of their particles, but also from their temporary display of rents during the application of a high temperature."

The paper concludes with a number of observations upon the porous nature of glass to water as well as light. This, he thinks, is proved by the experiments with empty bottles, well closed at the mouth, which have often been sunk in the ocean, and brought up full of water. In one, performed by Mr. Perkins, "the coverings were taken off layer after layer, but no signs of moisture were visible;" yet the bottle was full of water. In one experiment, by Mr. Grant, an empty bottle secured with a cork, wax, layers of oil cloth, &c. came up full of salt water; and in another experiment, a bottle with fresh water, similarly secured, came up filled with salt water: in both the layers of oil cloth, &c. were dry, and the cork, when cut across, presented no change.

ARTICLE XVI.

SCIENTIFIC INTELLIGENCE, AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Oil obtained by Distillation from the Hop.*

(To the Editor of the *Annals of Philosophy.*)

SIR, *Sept.* 21, 1821.

In Dr. Ives's communication on the Properties of the common Hop (*Annals of Philosophy* for March), I was surprised to observe it asserted, "that no essential oil can be obtained by distillation in any portion of the hop," having, in the course of experiment, obtained a considerable quantity; the only difference being in the quantity of hops submitted to distillation, six or ten pounds, instead of as many ounces, in the case of Dr. Ives.

In the kiln-drying of hops for sale, a portion of the essential oil is dissipated in vapour. It is, therefore, advisable to procure hops recently picked, at the present season of the year (before they are placed on the kiln), when, by distillation from ten pounds (with ten gallons of water) placing in the receiver or separator, a saturated solution of alum, the essential oil will saturate. I am, Sir,

Your obedient servant,

J. T. S.

II. *Process for extracting Cinchonin from Cinchona.*

The following process is given by M. Badolier (Ann. de Chimie et de Physique, tom. xvii. p. 273): A pound of yellow bark, bruised, is to be boiled in three pints of a very dilute solution of caustic potash (*eau alcalisé par la potasse caustique*). After the ebullition has continued for a quarter of an hour, the liquor is to be suffered to cool, and strained through a fine cloth with pressure; the residuum is to be repeatedly washed and pressed.

The cinchona thus washed is to be slightly heated in a sufficient quantity of water, adding gradually muriatic acid until litmus paper is slightly reddened, and stirring the mixture. When the liquor is near the boiling point, it is to be strained, and the cinchona strongly pressed; then add to the strained liquor while it is hot, an ounce of sulphate of magnesia; after this, precipitate the whole with caustic potash, slightly in excess. When the liquor is cold, the precipitate is to be collected on a filter, washed, and dried, and then treated with alcohol, as directed by MM. Pelletier and Caventou, in order to obtain the cinchonin.

When sulphuric acid is added to the cinchonin immediately after the separation of the alcohol, crystals of sulphate of cinchonin are obtained, which, when washed with a little water, are of a very fine white colour.

III. *Analysis of the Sulphates of Cinchonin.*

M. Robiquet has analyzed three sulphates of cinchonin in the following manner, and with the annexed results. Equal quantities of each of the sulphates, which had been dried by the heat of a salt water bath, were dissolved in distilled water; and to each solution similar quantities of pure caustic potash are added. This decomposition effected with heat is attended with some peculiar characters. The solution becomes at first milky; and afterwards small oily drops swim on its surface; at last, when the heat has been long continued, the cinchonin coagulates, and unites into large white masses, which are opaque and very porous, and when these occur, the decomposition is complete. In order to finish the analysis, it remains only to filter the liquor, to separate it from the cinchonin, to wash the filter in the common manner, to supersaturate it with nitric acid, and to add a small quantity of nitrate of barytes.

The sulphate of barytes thus obtained indicates, of course, that of the sulphuric acid contained in the salt under examination.

By this process, M. Robiquet analyzed the acidulous sulphate of the third crystallization, subsulphate of the first crystallization, and subsulphate of the third. The cinchonin, separated from the acid, did not contain any portion of sulphuric acid.

100 acidulous sulphate of cinchonin, third crystallization:

Sulphuric acid	19·1
Cinchonin	63·5
	82·6

100 subsulphate of cinchonin, first crystallization:

Sulphuric acid	11·3
Cinchonin	79·0
	90·3

100 subsulphate of cinchonin, third crystallization:

Sulphuric acid	10·0
Cinchonin	80·9
	90·9

(Annales de Chimie et de Physique, tom. xvii. p. 320.)

IV. *Purification of inferior Brazil Woods.*

Dr. Dingler observes, that the true Brazil wood being extremely scarce and dear, it may be interesting to state a method of substituting inferior kinds for it; those enumerated are Bois de Bimas, Sainte-Marthe, d'Aniola, de Nicaragua, de Siam ou de Sapan, &c. These woods contain fawn-coloured matter, which deteriorates the lustre of the red; this inconvenience is remedied by the following process: The rasped woods are to be boiled in water until all their colouring matter is dissolved. The decoction procured is to be evaporated until it equal to about 12 or 15 times the weight of the wood employed. In about 12 or 18 hours after the evaporated liquor is cold, skim milk, equal in weight to half the quantity of wood used, is to be added to it. When the mixture has been well stirred, it is to be boiled for a few minutes, and then strained through a fine piece of flannel. It will be then seen that the fawn colour will attach itself to the cheesy matter of the milk, and it is afterwards precipitated, without occasioning the slightest loss of the red colour.

In order to employ the liquor thus obtained in dyeing, it is to be merely mixed with a proper quantity of pure water; but if it is to be used for printing calicoes, it must be further evaporated; so that one part of wood shall yield only five or six parts of decoction. To this is to be added some starch, or other substance, to thicken it properly, and a sufficient quantity of solution of tin, or some other base, to brighten its colours. When printed, the colour is nearly or quite equal to that of the true Brazil wood.

The quantity of skim milk employed must always be in proportion to the quantity of colour contained in the wood made use of; one-third, or a fourth, of their weight will be sufficient for young woods, as they are poor in colour. The evaporation must also be regulated by the same principle, when intended for printing.—(Annales de Chimie et de Physique.)

ARTICLE XVII.

NEW SCIENTIFIC BOOKS

PREPARING FOR PUBLICATION.

The Study of Medicine, comprising its Physiology, Pathology, and Practice, in Four Volumes, 8vo. By John Mason Good, MD. FRS. Mem. Am. Phil. Soc. and FLS. of Philadelphia. These volumes, in addition to that lately published on Nosology, and dedicated, by permission, to the College of Physicians, will complete the Author's design; and constitute an entire body of Medical Science, equally adapted to the use of Lecturers, Practitioners, and Students.

No. I. The Genera of recent and Fossil Shells, intended as a Manual for the Use of Students in Conchology and Geology, with illustrative original Plates, by J. Sowerby, FLS. &c. &c. the descriptive Part and Observations by G. B. Sowerby, FLS. &c.

Practical Observations on Paralytic Affections, St. Vitus's Dance, &c. with Cases, by W. T. Ward.

JUST PUBLISHED.

A History of Brazil, comprising its Geography, Commerce, Colonization, Aboriginal Inhabitants, &c. By James Henderson. 4to. 3*l.* 13*s.* 6*d.* with Plates and Maps.

A Treatise on Dyspepsia, or Indigestion; with Observations on Hypochondriasis and Hysteria. Second Edition, enlarged by James Woodforde, MD. Castle Cary, Somerset. Crown 8vo. 5*s.* boards.

Hooker's Botanical Illustrations. Part I. Oblong 4to. 6*s.* plain; 10*s.* 6*d.* coloured.

ARTICLE XVIII.

NEW PATENTS.

Frederic Mighells Van Heythuysen, of Chancery-lane, London, for a new method of propelling small vessels or boats through water, and light carriages over land.—July 23, 1821.

David Barclay, of Broad-street, London, merchant, for a spiral lever, or rotary standard press. Communicated to him by a foreigner residing abroad.—July 26.

Thomas Barker, of Oldham, Lancashire, and John Rawlinson Harris, of Winchester-place, Southwark, hat-manufacturers, for certain improvements in the method of cleaning furs and wools, used in the manufacture of hats, from kemps and hairs.—July 26.

John Richard Barry, of the Minories, London, for certain improvements on, and additions to, wheeled carriages.—July 26.

Samuel Bagshaw, of Newcastle-under-Line, Staffordshire, for a method of forming and manufacturing vases, urns, basins, and other ornamental articles, which have been heretofore usually made of stone or marble, from a combination of materials never heretofore used.—July 26.

ARTICLE XIX.

METEOROLOGICAL TABLE.

1821.	Wind.	Barometer.		Thermometer.		Evap.	Rain.	Hygr. at 9 a.m.	
		Max.	Min.	Max.	Min.				
8th Mon.									
Aug. 1	S W	30·11	29·94	74	52	—		94	
2	S W	30·11	30·10	78	56	—	11	84	
3	E	30·10	30·09	76	52	—		84	
4	S	30·10	30·00	78	50	45		82	
5	S	30·00	29·90	84	53	—		74	
6	N W	29·99	29·90	76	52	—		83	☽
7	N W	29·99	29·89	71	51	—	05	77	
8	S W	29·89	29·59	66	56	45	36	94	
9	N W	29·59	29·56	70	52	—		84	
10	S W	29·65	29·56	67	52	—		76	
11	N W	29·91	29·65	68	45	47	09	78	
12	N W	30·01	29·91	71	49	—		75	
13	S W	30·00	29·82	74	57	—	11	81	●
14	W	29·97	29·82	68	48	—	16		
15	N	30·08	29·97	74	60	—		90	
16	W	30·15	30·08	76	61	45		90	
17	N W	30·15	30·05	75	60	—		87	
18	N W	30·19	30·15	73	46	—		92	
19	S	30·21	30·19	75	45	—		85	
20	E	30·21	30·18	79	45	43		79	☾
21	E	30·18	30·16	83	51	—		82	
22	N E	30·16	30·08	78	53	—		96	
23	N E	30·08	29·98	78	54	—		86	
24	S E	29·98	29·94	83	56	51		88	
25	N W	30·00	29·98	84	55	—	09	91	
26	N E	30·16	30·00	74	54	—	—	81	
27	E	30·16	30·01	63	56	—	02	79	○
28	E	30·01	29·84	60	52	—	30	80	
29	E	29·84	29·80	65	56	—	77		
30	S W	29·80	29·75	77	59	—	03		
31	S W	29·92	29·75	75	60	43	07		
		30·21	29·56	84	45	3·19	2·16	96—74	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

REMARKS.

Eighth Month.—1. Fine. 2. Morning, fine: showery in the afternoon. 3, 4, 5, 6. Fine. 7. Cloudy. 8. Rainy. 9, 10. Fine. 11. Morning, fine: rain in the afternoon. 12, 13. Fine. 14. Rainy. 15—17. Cloudy. 18. The sky this morning was obscured by a haze, through which the sun appeared of a pale blue colour, resembling, in some degree, the flame of sulphur, or of a Bengal light. This phenomenon was observed in several distant places. I have been informed that it was noticed in Essex and Worcestershire, and by many persons about London. I saw it in Sussex, where it lasted from about nine till near noon, and appeared nearly of the colour of watch-spring steel, and was occasionally hid by *Cirrostrati*, which were floating about. It may be noticed, that the weather, which had been for some time unsettled, cleared up the next day, and continued fine and very warm for about a week. 19—25. Fine. 26, 27. Cloudy. 28. Rainy. 29. Ditto. 30. Fine. 31. Drizzling rain.

RESULTS.

Winds: N, 1; NE, 3; E, 6; SE, 1; S, 3; SW, 7; W, 2; NW, 8.

Barometer: Mean height

For the month	29·973 inches.
For the lunar period, ending the 20th	29·943
For 13 days, ending the 1st (moon north)	29·865
For 14 days, ending the 15th (moon south)	29·898
For 13 days, ending the 28th (moon north)	30·087

Thermometer: Mean height

For the month	63·564°
For the lunar period	62·637
For 32 days, the sun in Leo	62·828

Evaporation	3·19 in.
Rain	2·16
Mean of hygrometer	84°

Laboratory, Stratford, Ninth Month, 20, 1821. **R. HOWARD.**

ANNALS

OF

PHILOSOPHY.

NOVEMBER, 1821.

ARTICLE I.

On Electro-magnetism. By Professor Oersted.
(Communicated by the Author.)

(A.) *The History of my previous Researches on this Subject.*

WHEN I began to examine into the nature of electricity, I conceived the idea that the propagation of electricity consisted in a continual destruction and renewal of equilibrium, and thus possessed great activity which could only be explained by considering it as a uniform current.[*] I then regarded the transmission of electricity as an electrical conflict, and my researches into the nature of the heat produced by the electrical discharge, particularly led to the conclusion, that the two opposite electrical forces, which pervade a body heated by their effect, are so blended as to escape all observation, without however, having acquired perfect equilibrium,[†] so that they might still exhibit great activity, although under a form of action differing entirely from that which may be properly termed electrical. Notwithstanding my efforts to justify my idea, this complete annihilation of power indicated by the electrometer, accompanied with very considerable action of another kind, appeared to the greater number of philosophers to possess but little probability. This feeling may, perhaps, be partly attributed to the obscurity of the subject, and partly also to the imperfect manner in which I explained my theory; for it must be confessed that new views are rarely developed even to their authors with perfect clearness. A thorough conviction of the agreement of my theory with facts, inspired me, nevertheless, with so strong a persuasion of its truth, that upon this basis I ventured to form my theory of heat and light, and to attribute to these forces, apparently destroyed, a radiating action capable of penetrating to the greatest distances. Having for a long time considered* the powers which are developed by electricity as the general powers of nature, it necessarily followed that I should derive magnetic effects from them.†

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[*] My treatise on this subject will be found in Gehlen's Journal 1806, and the Journal de Physique of the same year.

[†] See my Considerations on Natural Chemical Laws. Berlin, 1812, p. 132—234.

In order to prove that I admitted this consequence to the utmost extent, I cite the following passage from my Researches into the Identity of Electrical and Chemical Powers, printed at Paris in 1813, "it must be determined whether electricity in its most latent state has any action upon the magnet as such."‡ I wrote this during a journey, so that I could not easily perform the experiments, besides which, the manner of making them was not at that time at all clear to me, all my attention being directed to the development of a system of chemistry. I still remember that I expected, though somewhat vaguely, the effect in question, and particularly by the discharge of a strong electrical battery, and also that I did not hope to obtain more than a weak magnetic effect. Thus I did not follow the idea which I had conceived with the requisite zeal, but the lectures which I delivered upon electricity, galvanism, and magnetism, during the year 1820, recalled it. My auditory consisted mostly of persons previously well acquainted with the science. On this account, these lectures and preparatory reflections, led me on to deeper researches than those which are admissible in common lectures.

My original persuasion of the identity of electric and magnetic powers were developed with greater clearness; and I resolved to submit my opinion to the test of experiment, and the preparations for it were made on a day in the evening of which I had to deliver a lecture. I there showed Canton's experiment on the influence of chemical agency on the magnetic state of iron. I requested attention to the variations of the magnet during a storm, and I mentioned at the same time the conjecture, that an electrical discharge might produce some effect upon the magnetic needle placed out of the galvanic circuit. I immediately resolved to make the experiment. As I expected the greatest effect from a discharge producing ignition, I inserted at the place under which the needle was situated, a very fine platina wire between the connecting wires. Although the effect was unquestionable, it appeared to me nevertheless so confused that

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* See the letter added to my German publication, Materielen muner Chemie, &c. 1803; and also Researches into the Identity of Electrical and Chemical Forces, p. 127, &c.

† Letter, p. 234—238.

‡ Letter, p. 238.

I deferred a minute examination of it to a period at which I hoped for more leisure.*

At the beginning of the month of July, my experiments were repeated and continued without interruption until I obtained the results which have been published.

(B.) *Explanation of the first Law of Electro-magnetic Effects.*

The electro-magnetic effect which I discovered by the aid of the galvanic apparatus, has since been produced by common electricity, so that the expression of electro-magnetic effect is perfectly justified by experiment. It is well-known that for the first experiments on this subject we are indebted to M. Arago, who has been equally successful in enriching both physics and astronomy with his discoveries; the illustrious President of the Royal Society of London has also made an important series of experiments on this subject.

I shall here state, rather more in detail than I have done in my first publication, the rule by which I think all electro-magnetic effects are governed. It is this: *When opposite electrical powers*† *meet under circumstances which offer resistance, they are subjected to a new form of action, and in this state they act upon the magnetic needle in such a manner that positive electricity repels the south, and attracts the north pole of the compass; and negative electricity repels the north, and attracts the south pole;*‡ *but the direction followed by the electrical powers in this state is not a right line, but a spiral one, turning from the left hand to the right.* Many philosophers, and some of them of great merit, have thought the spiral motion of electrical powers to be improbable. I shall endeavour in the sequel to show, that this supposition is less arbitrary than it may appear to be at first; but to prepare for this, it is necessary first to explain the meaning of this supposition, and then to prove that all electro-magnetic phenomena so completely harmonize with the rule given, that it will suffice even to anticipate those among them which were not known before experiment. I have not discovered so perfect an agreement with facts in any other theory which has been hitherto advanced. When I have shown that the rule is quite sufficient to comprehend all the facts under one point of view; that is to say, that it is a correct rule, I shall invite the reader to examine with me, whether it may not also be *a law*, according to which the phenomena are arranged in nature.* It is extremely difficult, and especially for those who are not much accustomed to the representation of complicated figures, to understand the spiral quite clearly. What I am going to state will be most readily understood in the following manner:

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* All my auditors are witnesses that I mentioned the result of the experiment beforehand. The discovery, therefore, was not made by accident, as Prof. Gilbert has concluded from the expressions which I made use of in my first announcement.

† I here repeat what I have already stated in other works, that by *electrical forces*, I mean only the unknown cause of electrical phenomena, whether it belong to imperceptible matter or independent motion.

‡ In my first memoir, I grounded all explanations upon the repulsions only which are exerted by electrical and magnetic forces; but I soon discovered, that from the fear of assuming more than the phenomena required, I drew an unjust inference; for if magnetic forces are the same as electrical under another form of action, it follows, that opposite forces ought to attract each other reciprocally, and forces of the same kind to repel each other.

Upon a slip of paper (Plate X), fig. 1, draw the line A B which is to be longitudinally divided into two equal parts; draw some small triangles so that the summits and the middle of the bases may be cut by the line. Put the sign + at that end towards which the summits point, and the sign — at the end towards which the bases are placed. This piece of paper is to be twisted round a quill, a piece of glass tube, or any other cylindrical body, in such a manner that the triangles, reckoning from the summit to the base, shall be placed from the left to the right hand of the observer. The cylinder enclosed in this manner I call the electro-magnetic indicator. With this indicator, that part of the connecting wire is compared whose effect is to be judged of, by imagining it to be put in the place of the latter in such a position that the end marked + may receive the electricity of the positive extremity of the galvanic apparatus, and the end marked — of the negative. This being done, it will always be found that the south pole of the suspended needle is repelled by negative electricity. For brevity's sake, we will designate positive electricity by + E, and the negative by — E. But when these forces have assumed their new condition, in which they possess no action upon the electrometer, and affect the magnet, we shall call them electro-magnetic forces, and denote them by the Greek letter + ε and — ε.

It would be useless to repeat upon this occasion the description of all the experiments mentioned in my Latin memoir; it will be sufficient to say, that with the assistance of the electro-magnetic indicator, all the effects of the connecting wire in the most dissimilar positions which I have described, may be anticipated; I shall mention one example, as it may make the subject clearer. Place a part of the connecting wire perpendicularly opposite the magnetic needle, and let the upper part of the conductor receive the electricity of the negative end of the galvanic apparatus; that part of the needle, which receives the effect, will turn towards the east. A, fig. 2, represents the horizontal section of the conductor with the signs above-mentioned to describe the direction of the electro-magnetic forces. B represents a magnetic needle, whose north pole turns towards the connecting wire. C is another which presents its south pole to the wire. Both will be directed towards the east; the north pole by — ε, which comes from the west; the south pole by + ε, which comes also from the west. It will be readily seen that the attraction caused

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* I had intended to develope this matter in the present memoir, but the desire of stating all I am able to say upon so difficult a subject with all possible clearness, has induced me to defer it to a future opportunity.

Fig. 1.

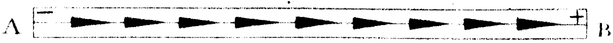
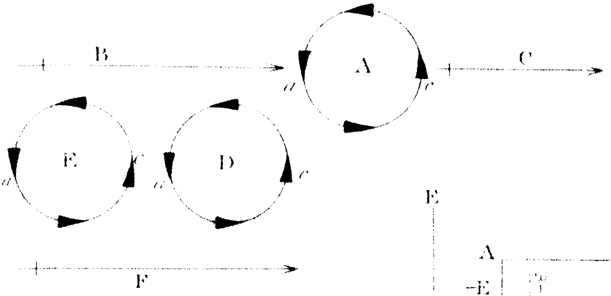


Fig. 2.



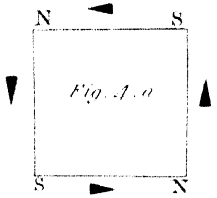


Fig. 4. a

Fig. 4. b

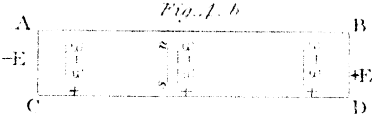


Fig. 3.

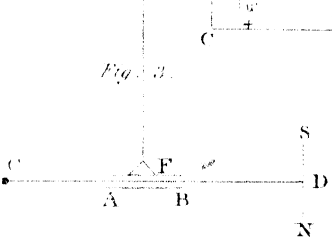


Fig. 5

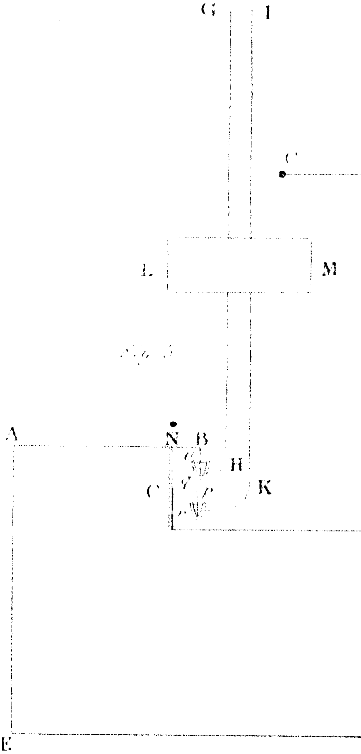


Fig. 6

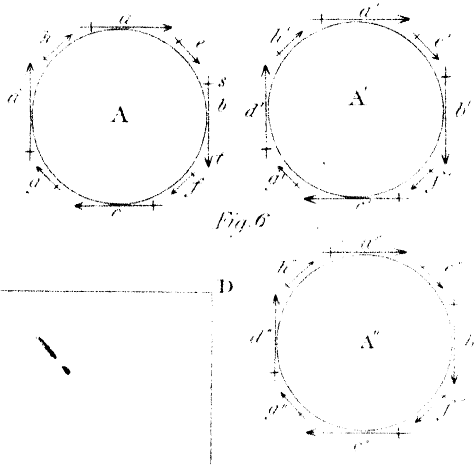


Fig. 7.

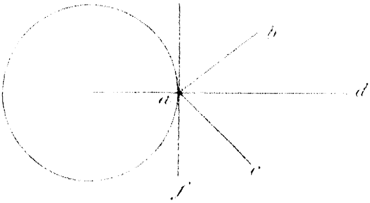
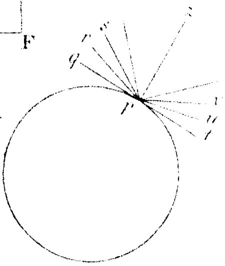


Fig. 8.



by + ε upon the north pole, and by − ε upon the south pole, must increase the motion towards the east.

If the conductor be placed opposite a point in the northern half of the needle, − ε in *a* directed towards the east will not act upon the north pole of the needle; while the contrary will be the case with − ε in *c* directed towards the west. It is true that the attraction of + ε in *a* will draw the needle towards the east; but it will act upon a very weak magnetic point, and, consequently, with but little power; the north pole will then go to the west. I do not reckon upon the advantage which the lever − ε in *c* possesses over + ε in *a*; for it may be readily shown that this advantage is not necessary to produce the phenomenon in question; it requires only that a magnetic needle should be fixed perpendicularly to one of the ends of an arm of a torsion balance, and that it should be arranged with the galvanic connecting wire. When the conductor is so placed that E represents a horizontal section of it, − ε in *c* will exert some attraction upon the south pole of the needle, but + ε in *a* repulses it with greater force; this end will, therefore, turn towards the east. I have already shown by the effect of A upon C what will happen when the conductor is opposite the south pole. It may also be readily foreseen, what will happen when the conductor is placed on the western side of the needle *p* E with respect to the needle F. When the conductor is at D, − ε of *a* will repel the north pole, but + ε of *c* will attract it with more force, the north pole will then turn towards the west, whether the perpendicular conductor be placed on the eastern or the western side. In the same manner, the south pole will in both cases turn towards the west, which will be readily explained by referring to the figure.

If any one be desirous of seeing the contrary effect of the two sides of the connecting wire in a more direct manner, it is only requisite to give a moveable needle the same magnetism at both ends. It is not that I consider this experiment as necessary after so many similar ones, but it may, perhaps, be very useful to represent the thing in the most simple manner in an elementary lecture. With the same needle the experiment may be made with the connecting wire placed horizontally opposite the ends of the needle, and observe the movements from the top to the bottom, or the bottom to the top. In this way it is possible to show in a manner very easy to be understood, the direction of the electro-magnetic forces in the connecting wire; it is also very easy to perform electro-magnetic experiments by the following arrangement: Let A B, fig. 3, represent a small hollow cylinder, which may be made of paper, and in this a very small cylinder of wood must be made to turn with but little friction. The end D carries the needle S N, and the whole is fastened with a hair or a very fine metallic wire. When the effect of the connecting wire has been tried upon N, it is only necessary to turn C D in the cylinder, so that N may be uppermost; and then the effect of the other side of the needle may be immediately tried. It is also easy to place S N horizontally, and to try the attraction or repulsion exerted upon it by any given part of the connecting wire.

Many ingenious attempts have been made to explain electro-magnetic phenomena. The first which I am acquainted with is that of my illustrious friend Berzelius. This philosopher supposes, that the galvanic conductor possesses double transverse magnetism, so that a conductor in the form of a parallelopiped has a north magnetic pole at one of its angles, and a south at the other. Let N S, N S, fig. 4 *a*, represent the transverse section of such a conductor placed in the magnetic meridian, and receiving the current of electricity from the positive end of the pile in the direction of south to north. The letters N N denote the two north poles; the letters S S the two south poles of the conductor. This theory explains many of the phenomena satisfactorily, and with surprising facility, as might naturally be expected in the hypothesis of so distinguished a philosopher, but it agrees nevertheless with only a part of the phenomena. The observation which I have so frequently had occasion to make in my experiments, that round conductors act in so equable a manner in every part of the periphery, that no distribution of poles is discoverable in them, excited some suspicions against this new hypothesis, and a direct experiment decided me absolutely against it. Twist a steel wire round one half of a square conductor, in such a manner that it may coincide with the semiperiphery N S N, or S N S, on which side soever it may be; according to the hypothesis, this wire ought to have either no magnetism at all, or equal poles at the two ends; but it will be found that the wire has always a north pole at that point towards which $-\varepsilon$ is directed, and a south pole at the point to which $+\varepsilon$ is directed. These directions will be explained in fig. 4.

As in these experiments very fine wire only should be used, a weak needle should also be employed: a small piece of the same iron wire fastened to a bit of raw silk is extremely convenient. In general, a steel wire may be magnetized by placing it across the conductor, although the latter be a parallelopiped, round or flat, and the wire may occupy a great or small part of the periphery; the point towards which $-\varepsilon$ turns always gains the property of turning towards the north. What is also remarkable is, that the magnetic pole produced in the steel wire applied to the conductor, is of the same kind as the pole of a neighbouring magnet repelled in the same direction. This proves also that the conductor cannot be considered as a body which has distinguishable poles on the surface; for in this case the poles produced and repelled would be of the same denomination.

In order to answer the question, whether the wire attached to the surface of the conductor might be considered as a part of that surface, differing only from others in its power of retaining the magnetism communicated, I put a piece of fine paper between the conductor and the steel wire: in other respects I performed the experiment as before. I had the same result, with this difference only, that the effect was rather weaker.

When a light magnetic needle is placed upon a large conductor, through which a strong discharge is passed, its direction is almost entirely determined by the electro-magnetic effect, and the magnetism of the earth causes but a very slight direction. Let A B C D, fig. 4 *b*, represent the large conductor, and suppose $-\varepsilon$ to enter at A C, and $+\varepsilon$ at B D; the direction of the electro-magnetism may then be marked by the signs $+\varepsilon-\varepsilon$. Place a magnetic needle S N properly mounted above the conductor, and let us call the end of it which turns towards the south pole s, and that which turns towards the north n; this being done, it will be found that the direction $s\ n$ will coincide with the direction $+\varepsilon-\varepsilon$. If the needle held always in the same horizontal plane be put towards one of the sides of the conductor, the north pole will be repelled from the side A B, but attracted by the side C D, only much more feebly than before. The cause of this phenomenon is undoubtedly that every point in that half of the needle which turns to the north is repelled by $-\varepsilon$ coming from the south, and attracted by $+\varepsilon$ coming from the north. In every point of the conductor, there is, therefore, an effort to act magnetically in two different directions.

M. Prechtel, of Vienna, a distinguished chemist, has succeeded in representing the phenomena of the galvanic conductor by means of iron wire turned into a spiral form, which he touches with the magnet in the same manner as if he were magnetising a cylinder. This spiral thus gains transverse poles, but no sensible polarity from one end to the other. By employing the requisite means, each coil of the spiral has more than two poles given to it, and it will then produce the same effects as the connecting wire upon the magnetic needle. This experiment has led him to consider the connecting wire as a transverse magnet, having a great number of successive poles, which are alternately north and south. It will be observed that we have arrived by different routes at opinions which are almost entirely similar. I prefer, however, to keep the name of electro-magnetism for the state of the connecting wire; for in the first place, there is no distinct pole in such a conductor; and besides this, the continual production of fresh electricity in the galvanic apparatus requires that we should suppose the electro-magnetism to be continually renewed, and an uninterrupted circulation of electrical forces in the conductor. In order that magnetism, properly so called, may be exhibited, it is requisite that the circulation should be interrupted, without the contrary effects of the activity which existed in the conductor, being suspended.

(C.) *Explanation of the Attractions and Repulsions which Galvanic Conductors excite among each other.*

As soon as I had suspended a small galvanic apparatus in the manner of the torsion balance, I tried whether the connecting galvanic wire would act upon that of the suspended apparatus ; but on account of the too weak action and great weight of this apparatus, I had no sensible effect. The same thing has happened to several other philosophers who have tried the same process, as I have seen in several treatises published upon electro-magnetism. M. Ampère selected a better process. He made a moveable conductor which he communicated with an apparatus of considerable strength, and thus he succeeded in discovering the attractions and repulsions of the galvanic conductor. His memoirs upon electro-magnetism are already too well known to render it necessary for me to say, that this distinguished philosopher has evinced the same extraordinary sagacity in the application of the discovery, as in his preceding labours, all of which evince great penetration ; and if I adopt a theory of magnetism differing from his, I shall never cease to acknowledge the great merit of his labours.

My present apparatus for experiments upon the reciprocal effects of the galvanic conductor, appears to me to be sufficiently simple ; and I shall now describe it. A B C D E F, fig. 5, is the moveable conductor, made of brass wire, of one-fourth of a line in diameter ; N C is a small wooden cylinder, to prevent as much as possible any alteration in the form given to the brass wire. The two points, *o*, *p*, move in two conical iron cups, *q* and *r*, filled with mercury. In *q*, the point rests upon the bottom, and upon this, the whole of the conductor; in *r*, on the contrary, the point moves freely in the mercury. G H and I K are brass wires which support *q* and *r*. L M is a little bit of wood, in which these wires are inserted, and which, by means of a screw, are fastened to any support. When G and I are put into the requisite communication with the conductors of the galvanic apparatus, the wire A B C D E F forms a part of the communicating wire, and arranges itself in the direction of the magnetic east and west, as was discovered by M. Ampère, and may be subjected to the principal experiments upon the action which the connecting wires exhibit. But in order to render the effect imperceptible, which the conductors, designed only to convey electricity, produce upon the moveable conductor, it is requisite to make G H and I K a foot or more in length, and especially to prevent the conductors of the apparatus approaching the moveable conductor. It will be understood, that in more delicate experiments this apparatus may be inclosed in a glass case, provided only that the wires G H and I K are passed through it by means of a cork ; but for the greater number of experiments this precaution is not necessary.

It is to the well-conducted researches of M. Ampère that we owe the law, *that the conductors or parts of parallel conductors attract each other when they both receive the electric current in the same direction, and repel each other when they receive it in contrary directions.* He does not endeavour to derive this law from the nature of electric forces, but considers it as a law which is independent of any laws already known.

I shall show that this law is necessarily derived from that which I have discovered.

Let us regard the thing at first as if the effect of electricity upon the magnetic needle had not been discovered. The attractions or repulsions of conductors of which we see no trace, unless they are pervaded by electrical powers, can be attributed only to those powers, and they must have such a direction in the conductors as to enable them to produce the effects discovered.

Let us consider the various modes of action which may be conceived, in order to discover that which agrees best with the circumstances demonstrated by experience. Fig. 6 represents the transverse sections of two conductors, which receive the current in the same direction. Neither of the electrical forces can be in any sensible excess, for such an excess would cause the conductors to repel each other mutually. The effects of the two forces cannot either, possess the same direction; for in this case, they would destroy each other. Still less can any inequality be suspected in the states of the two conductors, because they are supposed to be equally, and in the same manner, pervaded by the two forces. Thus the forces must leave each point of the surface in opposite directions; consequently their direction cannot be in the lengthened radius, but each of the forces must follow the direction of one of the tangents opposite to the point from which they set out, $p e$, to the point C on the conductor A, — E will go towards t, while + E will go towards s. Let us call also in this place + E and — E, which act transversely, + ε and — ε, in order to distinguish them from the forces in the longitudinal direction, as besides they agree absolutely with what we have before marked with these letters.

If any one should adopt the improbable idea, that the forces leave each point in two opposite directions, which will be found on contrary sides between the tangent and the lengthened radius, as $a b$ and $a c$, fig. 7, each would nevertheless resolve itself into two directions, one of which would be $a d$, and would produce no effect in consequence of the union of the two forces, and the other would be for one force $a e$, and the other force $a f$, consequently the effect would depend upon direction in a tangent.

I have observed that this supposition is improbable, but it is very likely that the forces may act at the same time according to the tangent, and in every direction between the tangent and the lengthened radius, so that they may form pencils from the point p, fig. 8, + ε in the directions $p q$, $p r$, $p s$, &c.; — ε

in the direction $p\,t$, $p\,u$, $p\,v$, &c. but always so that $+\varepsilon$ remains on one, $-\varepsilon$ on the other side of the lengthened radius.'

No one will readily believe that the effect of a force can proceed from the surface under a different angle from the other, for suppose one to act in the direction of the long radius, and the other in another direction, that which would have the advantage of acting directly, would produce greater effect than the other, and the conductors would repel each other. It would be almost the same as supposing that one force approached more in the direction of the lengthened radius than the other; for this force would resolve itself into two others, one longitudinal, and the other radial; and the latter would be stronger than the radial effect of the other force.

The only supposition then by which the electrical forces can produce the described effects is, that they proceed from every point in such a manner that the directions of the opposite forces are separated by the lengthened radius. But in order to show it clearly by a figure, let us represent only the directions according to the tangent, and those of some particular points, which may be exhibited as examples of what passes in others. When we consider the effects which take place in the longitudinal directions, fig. 6, where the analogous points are marked by the same letters, it will be seen that the direction of $-\varepsilon$ of a meets that of $+\varepsilon$ of a'. In the same manner $+\varepsilon$ of c, and $-\varepsilon$ of c' meet each other. This meeting of opposite forces which ought to produce attraction, occurs also in most of the points of the two peripheries, that of e in relation to h, f in relation to g. It is true that the points e and g, f and h, as well as the neighbouring points, repel each other; but on account of the small number of active points and the oblique direction, this effect must be much exceeded by the attracting effect.

A$''$, fig. 6, represents the transverse section of a conductor, in which the directive effect of the electricity is opposed to that of A and of A$''$. The points d and d'' repel each other on account of $+\varepsilon$, and the points C and C$''$ on account of $-\varepsilon$. Besides this there exists also here repulsion between all the points which are respectively in the same situation as the points which in the first case attract each other. The attracting effect which g and $e,''$ f and h'' produce, as well as of the neighbouring points, is here overcome by the repulsive forces, in the same way as in the preceding case the repulsive effect, was overcome by the attracting forces.*

Although these conclusions are expressed only as it is requi-

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* I have already given this explanation in a public lecture on Jan. 2, in this year. The first Tuesday in every month I give a public lecture, in which I announce new discoveries as they come to my knowledge. At the same time I demonstrate their relation to the system of our physical knowledge. It was in one of these lectures that I explained my opinion of the cause of the attractions and repulsions of the connecting conductor.

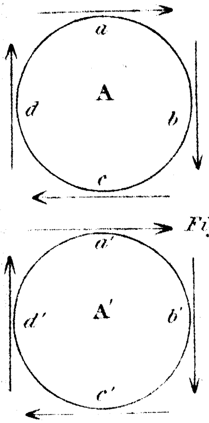

Fig. 9.

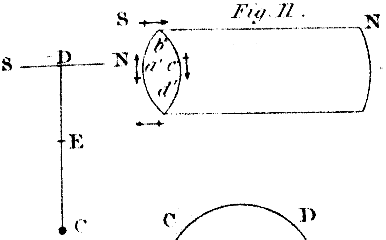

Fig. 11.

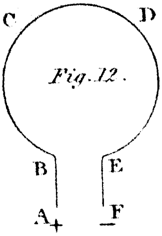

Fig. 12.

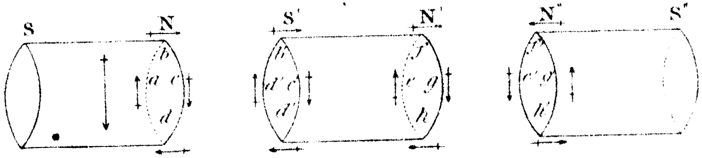

Fig. 10.

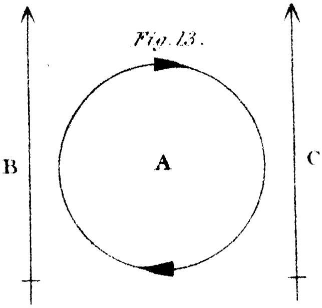

Fig. 13.

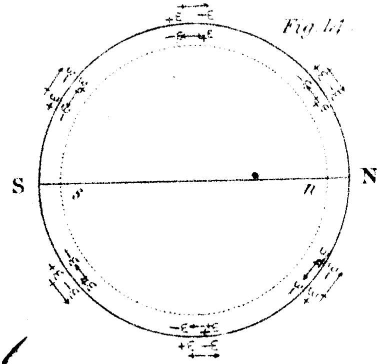

Fig. 14.

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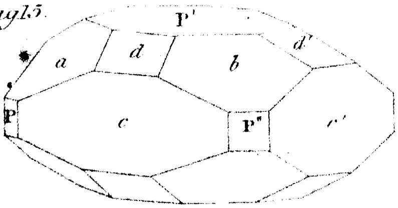
Fig. 15.



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site to do when treating of cylindrical conductors, they may nevertheless be readily applied to conductors of other forms; it appeared to me that the simplest representation should be preferred.

(D.) *The Magnetic Needle.*

As fresh electricity is continually evolving in the galvanic column, the discharge of it must be regarded as a continual addition and subtraction. The peculiar state of forces which exists in the connecting wire, in which they act as electro-magnetic forces, appears to me to be a state of continual agitation. But in the magnet, the mode of action of the same forces differs from that of electro-magnetism, in their being almost entirely in a state of repose, and forming no close circle. Here we must alter the denomination $+ \varepsilon$ into that of $+ m$, and the denomination $- \varepsilon$ into that of $- m$. The pole of a magnet towards which $+ m$ is directed ought then to produce the most marked effect of $+ m$, and the pole directed opposite to $- m$, ought, in the same way, to exhibit the strongest effect of $- m$, supposing always that the extension and the state of the conducting property of the conductor occasion no exception. We are now speaking of the effect of each point, and not of the greatest effect of the whole half of a magnet, which evidently can take place only opposite the end. In a certain sense it may be said that the magnet is a body charged with electro-magnetism. This manner of considering the magnet agrees with that generally admitted from the point at which we changed the expressions from $+ \varepsilon$ and $- \varepsilon$ to those of $+ m$ and $- m$; it may now be left without further explanation.

But it will be necessary to explain in this place the reasons which prevent my adopting of M. Ampère's ingenious theory of magnetism. It is well known that this philosopher supposes that the line which unites the opposite poles of the magnet, is surrounded with electrical currents placed in planes perpendicular to the axis, so that these currents, and not the longitudinal magnetic distribution, are the cause of magnetism. According to this idea, two neighbouring moveable magnets, would have a tendency to turn in such a manner, that their circular currents would attract each other. If then we place two magnets, one of which at least should be moveable, one above the other with their axes parallel, it must happen that the moveable needle will turn, until the opposite poles are placed upon each other. A and A', fig. 9 (Pl. XI), represent transverse sections of two magnets placed upon each other in the same direction; so that thus we see only one of the currents of each magnet. The darts do not represent the circular movements of the forces in the conductors $(+ \varepsilon$ and $- \varepsilon)$, but the direction of the current, such as it is usually imagined to be (+ E and − E). The similar letters in the two circles represent those places, in which the direction of the current is the same. Now the parts which have opposite directions of the current approach each other the most; and thus they will repel each other, until the needle has made half a turn, by which the currents are placed in a situation in which they may have parts in equal directions opposite each other, which cannot take place, excepting when the opposite poles shall be placed one above the other. In every other parallel position of the axes, the same relation occurs, as may be satisfactorily conceived by imagining the circles A and A′ to change their places in several ways.

S N, fig. 10, represents a magnet constructed according to this idea, and the darts have the same meaning as in the preceding figure, and will retain their meaning in the sequel. The direction of + E, or, according to M. Ampère, the direction of the electrical current, goes always to that side which turns towards us from bottom to top, as is indicated by the dart with a cross at the end, placed in the middle of the magnet S N. The right face of such a galvanic or electrical circle, always turns towards the north, the left side towards the south. S N thus representing a magnet, the end of the right side will endeavour to turn towards the north, or will be the north pole of the needle, and the end on the left hand will be the south pole. S′ N′, fig. 10, represents another needle like the former. When S′, the south pole of this needle, is placed opposite N, the north pole of the needle, so that *a*, *b*, *c*, *d*, of the latter are opposite *a*′, *b*′, *c*′, *d*′, of the former, it will be readily observed that parts, the currents of which have the same direction, are opposite each other, and must reciprocally attract each other, as occurs with opposite poles. If, on the contrary, S′ N′, fig. 10, be made to act upon S″ N″, so that N′ and N″ be opposite each other, and *e*, *f*, *g*, *h*, opposite the points *e*′, *f*′, *g*′, *h*′, opposite currents are placed together, and repel each other, as would occur with poles of similar descriptions.

To this point the explanation is perfectly correct; but when the magnets S N and S′ N′ are placed beside each other so that the end N of the first shall be beside the end S of the other, it will be conceived that the point *a* will be put in relation with *c* or *a*′ with *c*, or *d* with *b*′, or at last *d*′ with *b*; in all these cases, these currents meet, and must repel each other. Thus the theory is not in this case in unison with the phenomena. As that has been said of the relation of N to S′ may be applied to N′ and N″, the requisite changes being made.

A true magnet acts in the same manner with respect to a magnet constructed according to this idea. Let S N, fig. 11, represent the latter, and S′ N′ the former; but let S N be fastened to one end of a thin metal plate, D C, provided in E with a centre, and suspended like a magnetic needle. It will in fact be found that N is attracted by all the points placed in the interior of the circle *a*′, *b*′, *c*′, *d*′, but that it is repelled by all the points placed on the exterior of the circle, as ought to happen according to the theory which I have proposed, but it does not agree with the opinion, that S′ N′ should be considered as a magnet. As there is not in this case any question as to the length of the axis of the magnet, in making the experiment it is only requisite to employ a wire curved like A B C D E F, fig. 12, which receives + ε at A and − ε at F. The moveable magnet S N may also be mounted in the manner shown in fig. 3, only it is necessary to observe that C D should be so turned in the cylinder A B, that S N should be in a horizontal position.

The experiment cited by M. Ampère as particularly favourable to his theory, that a magnetic needle properly suspended is either totally attracted or repelled by the connecting wire in complete conformity with his theory, cannot be considered as contrary to mine, and has not moreover been so considered by M. Ampère, especially as he did not intend to criticise the theory which I had in few words proposed, and but very slightly explained; but the explanation of this account will increase the facility of comprehending that which I am here going to describe.

A, fig. 13, is a transverse section of an electro-magnetic conductor; B and C are two magnetic needles. It will be readily conceived that C will be repelled, and B attracted; for − ε in A will meet the north pole, and + ε the south pole of C; on the contrary + ε of A will meet the north pole, and − ε the south pole of B. This phenomenon is, therefore, a consequence which results with equal facility from both theories.

One of the difficulties of M. Ampère's theory is the explanation of the common manner of magnetizing steel, as has been already remarked by the celebrated Erman. It is unintelligible how by touching or rubbing the side of a steel wire, a galvanic circle can be placed around the whole of the wire. As the theory here sanctioned differs from the common one only in the derivation of the magnetism, and not in the idea which ought to be formed of the distribution of the magnetic forces in the magnet, the common theory may also be adopted. That which neither one theory nor the other can yet explain is the peculiar power which some bodies, especially iron, nickel, and cobalt, possess of receiving a high degree of magnetism, while almost all others are susceptible of it only in a very slight degree.

I shall not explain more at length my objections against the theory of the celebrated Ampère. If I have perfectly understood it, what I have already said will be sufficient. If I am deceived upon any point of his theory, I flatter myself that I have given him an opportunity of shedding new light upon this important matter.

(E.) *Magnetism of the Earth.*

The daily course of the light of the sun round the earth produces warmth, evaporation, and chemical agency, from the east to the west. From this also proceeds an alternation of the destruction and renovation of electrical equilibrium, and the effect of it must be similar to that of a galvanic circle applied round the earth. It is true that this electrical effect will be weak at each point, but the great extent of the electrical surface will abundantly supply it. The length of the circle or electrical belt is that of the periphery of the earth. The width of this belt extends nearly as far as the vicissitudes of night and day during a revolution of the earth. The width of this belt varies every day, since the diameter of the circle around the poles of the earth, during night or day, changes continually during several revolutions. For at 66° 32′ from the equator, there is once a year 24 hours of day, and 24 hours of night; at 67° 18′ there is a whole month of day light, and a month in which the night continues without interruption, &c. The mean width of this circle will extend but little further than the polar circle; for the violent changes which impede the regular progress of the weather, and consequently the regular effect of solar influence, exert great controul in the countries near the poles.

M. Ampère supposes that there is also an electro-magnetic effect round the earth from the east to the west, but he is of opinion that it belongs properly to the construction of the earth, although its revolution is not without some effect; he supposes also that there is no other magnetism of the earth, besides the immediate effect of electro-magnetism. As to this last point, my opinion also differs from that of the French philosopher. A body capable of becoming magnetic cannot be surrounded with an electrical current without receiving a magnetic charge. All bodies are susceptible of magnetism to a certain extent, although generally it is very slight when compared with iron. It follows then necessarily from the electrical circulation round the earth, that the earth itself becomes magnetic.

Let fig. 14 represent a section of the terrestrial globe supported by its two poles. Suppose + E in the electrical belt goes from the east to the west, and of course — E from west to east, — ε upon the surface of the earth goes then towards the north, and + ε towards the south; but the contrary direction must take place at the inferior side of this belt. The globe S *z* N *z* here represented as a nucleus surrounded by the crust which contains the electro-magnetic belt, will become magnetic, and at *n* it will possess the magnetic power which we find in that half of the magnetic needle which turns towards the south. It is thus that the magnetic needle receives its direction by the magnetism of the earth, and by the electro-magnetism of the surface. If the magnetic nucleus derived its magnetism from the electro-magnetism of the surface, its north pole would repel the same end of the needle that is attracted by the north side of the electro-magnetic belt; but as the lower plane produces opposite magnetism, the surface and the magnetic nucleus have the same effect with regard to the needle.

The intensity of action cannot be equal in the whole of the electro-magnetic belt of the earth, just as the effect of the sun is not the same upon the earth and the sea, and even differs according to the elevation of the country above the level of the sea. It is even possible that the different conducting power of different parts of the globe, particularly of land and sea, may possess great influence. It appears then that the new discoveries do not as yet furnish us with new facts sufficiently developed, to be useful in mathematical researches upon the situation of the magnetic poles of the earth. It is nevertheless to be wished that M. Hansteen, who has displayed so much solid learning in his researches into the magnetism of our globe (undertaken when he could not have recourse to the electro-magnetic discoveries), would resume his labour with the means which natural philosophy now offers. I shall confine myself to proposing some ideas to those who are willing to undertake a deeper examination of this subject.

According to the manner in which magnetism is produced on the globe S *z* N *z*, the strongest magnetic tension ought to occur in a circle round the ends *s* and *n* of the axis *s n*. We have hitherto supposed that the limits of the electro-magnetic belt were throughout equally distant from the poles of the earth. But there is reason to suppose that the electro-magnetic effect of the sun is but weak in those places which are covered with ice and snow during a great part of the year. It is then very likely that the electro-magnetic belt is very nearly parallel with the isothermal line of 0°. The form of the electro-magnetic belt determines also the form of the curve in which the greatest intensity of magnetism occurs round the poles of the globe *s z n z*. But the points of this line which are nearest to us would act most strongly upon our magnetic needles, and would appear as magnetic poles.

It must be confessed that in these conclusions we cannot always support ourselves upon evident principles; but I will, however, cite, as a remarkable confirmation, that the two magnetic poles, indicated by M. Hansteen, in the northern hemisphere are under the same meridian as the celebrated Humboldt, (who has rendered such great services to natural sciences), places the greatest concavity, that is to say, the greatest polar distance, from his isothermal line of 0°. I likewise remember to have heard M. Hansteen remark, several years since, that the magnetic poles are distinguished by extreme cold. We are now speaking of the northern magnetic poles, as to the southern hemisphere, we are in possession of too small a collection of facts to fix the isothermal line.

The annual and daily variations of the magnetic needle are intimately connected with the relation of the earth to the sun, but they do not appear to depend upon any variation in the intensity of the magnetism of the interior of the earth, by the electro-magnetism which the sun produces; for these variations do not occur upon different parts of our globe at the same time, in such a manner as they must do, if the variation depended upon the increase or decrease of the magnetic powers of the poles.

It is more probable that the electro-magnetic state of the surface of the earth determines these changes. Not having a sufficient number of observations upon this subject to found principles upon, nor having sufficiently developed the principles of the electro-magnetism of the earth to be able to arrange the observations which we possess, we ought to content ourselves at first, with indicating the acknowledged analogy which the diurnal and annual variations of the needle, with the periods of the day and seasons.

I have framed and examined many hypotheses as to the cause of the variations of the needle without satisfying myself. The different direction which the electro-magnetic belt receives by the united action of the annual and diurnal motions of the earth, the yearly and daily variations which occur in the figure of the electro-magnetic belt, the discharges which may occur when the electro-magnetic effect is at its maximum, the inequalities which are produced by the different effects of the sun upon the land and the sea, are considerations that have not yet given me sufficient agreement with the phenomena which have been observed in different parts of the earth. The frequent and unforeseen variations of the magnetic needle seem to depend upon electro-magnetic discharges, of which we have not at present any experimental knowledge. Among such discharges I particularly reckon polar light, known by the name of aurora borealis. I do not, on this account, oppose the opinion of the celebrated Biot; for I think it very probable that these discharges occur in certain clouds. Tempests have also a well-known influence upon the magnetic needle, which no longer surprises us after having found magnetism in every electrical discharge. It appears to me also very probable that several discharges occur in the air, and, perhaps, even in the earth, without our perceiving them. Among other irregularities of the needle are those which embarrass persons who have geometrical operations to execute in the hot days of summer in the open air; these seem to be owing to such imperceptible discharges. I hope that in future the magnetic needle will be used as a meteorological instrument. Weak needles seem to be preferable for this purpose, because the directing power of the earth produces only a weak action upon them, while a neighbouring electrical discharge has a marked effect. This agrees perfectly with the observations of Cassini, according to which a weak needle was subject to many irregularities, which he did not observe with a stronger one. But I would, above all, propose strong needles in meteorological researches, suspended, however, in such a manner that the magnetism of the earth may possess but little or no influence upon it. In order, however, to determine the direction of the discharges, it is requisite to have needles differently suspended. The mode of suspension represented in fig. 3 is one of the most important in this respect, as the magnetism of the earth has no influence upon its position; it is the bending of the wire alone from which the needle is suspended, which determines its direction. Another needle suspended in the same manner, but in a horizontal position, would be of great utility. By similar means, it may, perhaps, be discovered by subterranean geometry, whether the galvanic effects in mines do not disturb the magnetic needles employed.

In another memoir, I shall endeavour to show that the circular movement of electrical forces in the conductor, which I have admitted as an hypothesis, results from the nature of electrical forces; and I shall also endeavour to give a new explanation to the opinion which I expressed several years since, upon the production of light and heat by the conflict of electrical forces.

ARTICLE II.

On the Black Enamel obtained from Platina.
By Mr. J. P. Charlton.

(To the Editor of the *Annals of Philosophy.*)

SIR,

IN my communication in the *Annals* for Sept. I stated the grounds upon which I concluded that in all cases the rose colour imparted by gold to enamels is owing, not to the oxide, as usually supposed, but to the regulus of the metal; and that the oxide, where employed, is by the process of firing reduced to the metallic state. Another method of procuring gold in a state of fine division has since occurred to me; viz. by separating it from an alloy in which it exists in small proportion. I therefore, dissolved a shilling in nitric acid, and having ground with a little flux the gold which separated, in the form of a black powder, obtained, as before, a rose-coloured enamel. Vitrifiation is not essential to the production of the rose colour; for having exposed a quantity of ground flint in a crucible with a little gold to a long continued and intense heat (not less than 110° of Wedgwood's pyrometer), I found the flint tinged throughout of a delicate rose colour.

In the same paper, I stated, that from the analogy between gold, silver, and platina, and from the easy reducibility of the latter, I was led to suppose it probable, that the black enamel obtained from platina owed its colour in like manner to the regulus, and not to the oxide. Mr. Cooper's process for obtaining the black enamel is to mix dilute muriate of platina with neutral nitrate of mercury, and to expose the precipitate to a heat only sufficient to raise the calomel: the result is a black powder, which, according to him, is the protoxide of platina, containing only 4·7 per cent. oxygen. This powder ground up with flux yields a beautiful black enamel; and Mr. C. states it as a most singular fact, that the oxide, when thus mixed with flux, is not reducible by the strongest heat. It appears so very improbable, that the mere circumstance of being diffused in a vitrified flux should enable a substance, so easily reduced as platina, to retain its oxygen at intense heats, that I formed a suspicion, that the platina in the black enamel must already be in the metallic state. My first step was to ascertain that oxide of platina, either alone or ground with flux, lost the same weight in the heat of an enameller's kiln (which I found to correspond with 6° or 7° of Wedgwood's pyrometer), that which was fired alone *appeared* to have suffered no change, being still in the form of a black powder. It was evidently then from this black powder, whether oxide or regulus, that the colour of the enamel was derived. This substance, when exposed to strong heat, lost its blackness, and the platina resumed its usual metallic lustre, *but this change was accompanied by no loss of weight*; consequently the black powder was already in the metallic state, however unlike in appearance to platina in its usual form.

I find that the oxygen is entirely expelled from platina at a heat below redness, but it is impossible to judge from the appearance of the substance, when the expulsion begins or ends. Hence, therefore, Mr. Cooper's error may be accounted for, as I conceive that what he considered to be pure oxide, containing only 4·7 per cent. of oxygen, was, in fact, a mixture of oxide and of regulus.

The black grains of iridium remaining after solution of platina, which, according to Mr. Tennant, are in the metallic state, though friable and without lustre, are also capable of imparting colour to enamel. The colour is a brownish-green, when much flux is used. I am, Sir,

Your most obedient servant,

J. P. Charlton.

* * *

I am uncertain whether the crystallization of metallic gold has been hitherto observed or not: I beg leave, therefore, to enclose a few minute specimens; they were obtained by boiling nitric acid upon mercury, which happened to hold a little gold in solution; when the mercury was completely dissolved, long crystallized filaments remained: they were much broken in extracting them from the matrass.

ARTICLE III.

On Floetz Formations. By Thomas Weaver, Esq. MRIA. MRDS. MWS. MGS.

(*Continued from* p. 254.)

FLOETZ.—SERIES II.

1. The *Weissliegende* of M. Freiesleben.

Calcareous Conglomerate and marly Sandstone.[*]

M. FREIESLEBEN has the merit of being the first who distinguished this formation in Germany, most preceding writers having either entirely overlooked, or given an imperfect account of it, among the latter of whom may be reckoned Lehman, Gerhard, Heim, &c.; while others have confounded it with the old red sandstone (rothe todtliegende), considering it as the uppermost bed of that formation, e. g. M. Voigt, and indeed almost all later geologists.

The weissliegende, as the first member of a new series, like the rothe todtliegende, the first member of an older series, varies also in its composition and structure, accordingly as, in the course of its extent, it comes in contact with, and reposes upon formations of an earlier era; upon the first floetz group, upon transition, or upon primary tracts.

In *Mansfeld* and *Sangerhausen*, its general character is that of a calcareous conglomerate, marly sandstone, indurated sandy marl, indurated slaty clay marl, or a fine grained, partly siliceous, sandstone. The calcareous base is usually of a yellowish or greyish cast, with iron-shot spots and streaks, and occasional brownish-red layers. Numerous scales of white mica, and single grains of white or dark coloured quartz and flinty slate, appear incidentally in the finer-grained varieties. The conglomerate itself consists of grains of quartz, hornstone, and flinty slate, of greyish and blackish colours, compacted by a slight marly cement, usually of a whitish cast, in which are lodged rounded and angular fragments of compact and sandy marl. The marly sandstone contains sometimes spheroidal and ovoidal pieces of compact limestone, and sometimes thin layers of bituminous marl shale.

Nearer toward the *Hartz*, in *Stollberg*, and the adjacent districts, the more common appearance of the weissliegende is that of a siliceous conglomerate of larger or smaller grain; the ingredients consisting of rounded and angular pieces of hornstone, flinty slate, lydianstone, clayslate, jasper, quartz, sandstone, limestone, calcareous ironstone, and iron ochre, with mica, and iron pyrites; not assembled all together in any one quarter, but differently grouped in different districts.

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[*] It may not be useless to notice in this place an error of the engraver in the tabular arrangement of M. Greenough's Geological Map. The "various sandstones, rarely calcareous; post, crowstone, ganister, pennant," which there occupy the lowest line of Group No. 17, ought to have been included in the circumflex of Group No. 18, namely, in the coal formation.

In the *forest of Thuringia*, also, this formation is more nearly allied to a siliceous than to a calcareous conglomerate; and at Ilmenau, it contains portions of hornstone porphyry of a flesh or brownish-red colour; while in the circle of Neustadt, the lowest portion of the coarse conglomerate being ironshot and coloured red, it has then a near resemblance to the rothe todtliegende; which circumstance has no doubt partly led to the confounding of the two formations together.

In *Hesse*, in the district of Riegelsdorf, the weissliegende consists of a coarse calcareous conglomerate, a base of grey clay marl enveloping rounded pieces of quartz, micaslate, hornstone, clayslate, jasper, felspar, and slaty clay marl, with scales of white mica, single laminæ of calcareous spar, and more rarely rounded pieces of compact limestone.

Generally speaking, the weissliegende, when partaking more of the character of marl than of sandstone, is more or less ironshot, and coloured yellowish or brownish.

To complete the distinctive characters of this formation, other minerals, occasionally entering into its composition, must also be noticed. Of these, calcareous spar is frequently found in disseminated laminæ, or collected in round masses, which are usually hollow and lined with crystals; granular and fibrous gypsum, sometimes alternating with it in thin layers, and specular gypsum in lenticular portions. Heavy spar is more rare. Mica is generally distributed. Grains of mineral pitch are found in it immediately below the copper shale, in the Sangerhausen district. Streaks and layers of pitch coal and slate coal, not exceeding half an inch in thickness, and mineral charcoal in angular pieces, several inches in circumference, have occurred only in the Groscamsdorf district.

In several tracts, the weissliegende is metalliferous for certain distances, the upper layers being impregnated, one or more inches in thickness, with metallic substances, principally ores of copper; but ores of cobalt have also appeared, beside iron pyrites, galena, native bismuth, blende, and copper nickel. In these cases, the copper shale commonly proves barren, but in some places both are found rich in metal.

The only instance of organic remains noticed in this formation is that of chamites, found near Riegelsdorf, at a depth of several fathoms below the copper shale.

In many tracts, the weissliegende forms only a slight bed, but in some quarters it becomes considerable, e. g. in the Riegelsdorf district, where its thickness varies from 70 to 130 feet.

The analogues of the weissliegende in England, are to be found in the calcareous conglomerates, or *popplestones*, of Devon, and of the Quantocks in Somerset,* where they repose chiefly, if not entirely, on transition tracts; in the calcareous and calcareo-magnesian conglomerates of the Mendips, being there frequently metalliferous, producing galena, calamine, and iron ore; in those of the vicinage of Bristol,† of the environs of Tortworth, and of the southern quarters of Monmouthshire and Glamorganshire; in all of which tracts they repose on the first floetz series; and such is also the general position of this formation in the north of England.‡

2. *Lower Limestone Formation of M. Freiesleben.*

Magnesian Limestone Formation.§

In Mansfeld, the lower portion of this formation is constant in its order of position, wherever the continuity is preserved; the marl shale reposing on the weissliegende, and being covered by the zechstein. But the members of the upper portion, lying above the zechstein, observe no regular order of succession, being generally disposed in a different order in different districts; sometimes also comprising one or more beds in one quarter, which are wanting in another.

The whole formation is distinguished by the presence of gypsum, disseminated in the lower portion, but in mass in the upper. A second characteristic of this formation is its general tendency to a porous or cavernous structure. And a third is to be found in its variable composition, as resulting from different proportions of lime, alumine, silex, oxide of iron and bitumen, differently intermixed and combined. It may also be remarked of this formation in general, that its thickness is variable, not only as a whole, but as respecting its different members.

Lower Portion.

Marl Shale, comprehending Copper Shale and Roof Shale.—The bituminous, black or brown, marl shale, which, from its frequent metalliferous, and in particular its cupriferous character, has obtained the name of *copper shale*, varies much in different quarters, and even in the same district, according to the relative proportion and intermixture of its earthy constituents with bitumen and metallic substances. In some places it passes even into bituminous clay marl. Its average thickness is from 10 to 20 inches, while the *roof or grey marl shale*, which forms the upper part of the bed, varies from four to eight feet in thickness. The dip of the bed of marl shale is generally most rapid near the outcrop, gradually acquiring a lower angle at a greater depth; but the position being very variable, it is found inclining nearly under all angles between the vertical and the horizontal.

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* See Mr. Horner's paper in Geol. Trans. vol. iii. p. 355, et seq.

† See the papers of Dr. Bright, Mr. Warburton, and Dr. Gilby, in Geol. Trans. vol. iv. p. 202—214.

‡ I am not aware of beds or masses of trap, porphyry, or amygdaloid, being found as a formation subordinate to this conglomerate and sandstone. The masses, adduced as such in Devonshire, seem, on the contrary, to protrude from, and to belong to, the transition tracts of that country, e. g. near Thorverton. (See the Rev. J. J. Conybeare on the Red Rock Marl, in the *Annals of Philosophy*, April 1821, and Mr. Greenough's Geological Map).

Is there any well authenticated instance of trap, porphyry, or amygdaloid, occurring as a subordinate member of the second or third floetz series of formations?

§ We are indebted to Prof. Buckland for the first indication of the identity of the English magnesian limestone formation, and that which was denominated in Germany the first floetz limestone by Werner. (See "Order of Superposition of Strata" appended to the "Geology of England and Wales, by W. Phillips, MGS." in 1818.)

In several quarters, the copper shale forms more beds than one. Thus, for example, in the forest of Thuringia, it occurs in the territory of Henneberg in several thin layers, sometimes separate, sometimes coalescing; and near Bennowitz it forms thin beds, several feet apart, lying in the ferriferous limestone, which there takes the place of zechstein, and is known by the name of gryphite limestone.

The non-metallic substances which occasionally appear in the copper shale, are: quartzy sandstone, in elongated compressed masses, six or eight feet in length, and three or four inches thick; compact limestone, in lenticular portions, or in layers; fibrous carbonate of lime, in frequent extremely thin layers, and calcareous spar more rarely, in strings, and lining drusy cavities, which sometimes appear several inches in extent; fibrous gypsum, in repeated thin layers, and, more rarely, foliated gypsum in thin laminæ; drusy quartz in small veins; mineral pitch, in a pure state, in layers several lines in thickness; coal, as the substance of animal and vegetable remains, and forming also lamellar masses, one foot long, and one-fourth to half an inch in thickness; mineral charcoal, very rarely, in pieces eight or nine inches long, and a half to one inch thick; and mica, in disseminated minute scales.

The principal metallic substances found in the copper shale, are ores of copper with iron pyrites, either intimately mixed or in perceptible portions; but beside these, blende, ores of cobalt and nickel, and galena, are also met with; while antimony, bismuth, arsenic, and native silver, are very rare. The ores are generally found in the form of thin layers, slight strings, in small nodules or grains, and disseminated; sometimes also membranous, or specular. But the native silver and native copper occurred in the filiform, capillary, or membranous state. The copper shale is extremely variable as to produce of metal in different parts of its course, being in some quarters not worth smelting, and in others altogether barren. In those parts that are sufficiently rich for metallurgic processes, the greatest thickness adapted to the furnace is nine or ten inches, and the smallest from two to five inches.

Of animal remains, the following have been found in the copper shale: skeletons, referred in general by Baron Cuvier to the genus monitor; of fishes, numerous impressions, skeletons more rarely, and single teeth occasionally; fragments of zoophytes, near Schmerbach in Gotha; trilobites, designated by Baron von Schlotheim as trilobites bituminosus, in the copper shale of Schmerbach, and also in that of Riegelsdorf; shells very rarely, but small gryphites,* tellinites, striated terebratulites, ammonites, and belemnites, have been observed, beside fragments of other shells.

Vegetable remains are much more scarce in the copper shale, and less determinate; such as impressions resembling lycopodia, ferns, leaves, ears of corn, the jointed stem of the bamboo, and seed vessels.

The *roof shale, or grey marl shale,* appears sometimes corroded, or vesicular, resembling scoria, the cavities being empty, or coated with a calcareous sediment, or encrusted with crystals of carbonate of lime. In some quarters it contains fine sandy layers, resembling rauhstein; in others, it approaches to the character of slaty swinestone, and in others to that of bituminous marl shale. Occasionally it contains also copper shale in thin streaks; fibrous gypsum, and calcareous spar, in layers, strings, and disseminated portions; iron pyrites, in thin layers and strings; and copper ores, disseminated for short distances. The only organic remains found in the roof shale are gryphites, recently met with near Hettstädt.

Zechstein.—A compact limestone, commonly thinly stratified, and more or less argillaceous, containing in Mansfeld about one-sixth part of alumine, and in Lower Saxony one-fourth: extremely tenacious, whence its name of *toughstone;* usually grey, but sometimes approaching to black; and occasionally vesicular, the cavities being coated with a ferruginous deposit. In general external character, it much resembles that compact variety of shell limestone, which has a splintery, even, or flat conchoidal fracture.† It varies in thickness from 14 to 112 feet, and in some quarters forms abrupt rocky hills; the strata being sometimes curiously inflected; but in some parts of the forest of Thuringia, it is wanting altogether, the ferriferous or gryphite limestone appearing in its stead.

Occasional intermixed minerals: calcareous spar in scattered folia, in strings, and lining drusy cavities, and fibrous carbonate of lime in layers two or three inches thick; gypsum, compact, granular, and specular, in nodules, or disseminated, and fibrous gypsum in layers and strings; clay galls rarely; quartz crystals with brown iron ochre, in nests and streaks; and in the lower strata, particles of mica.

Metallic substances: brown iron ore in thin layers or gæodes; clay ironstone, and black clay with disseminated iron pyrites, in nodules; and some copper ores, in disseminated grains. In Lower Saxony, also, galena, in nests and strings, beside traces of mineral pitch.

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* Gryphites aculeatus. (See Petrefactenkunde of Baron von Schlotheim.)

† Lias limestone.

Organic Remains.—Of these, the zechstein is generally free, containing them only in certain quarters. In Mansfeld, small terebratulites and gryphites have been observed; in the forest of Thuringia, gryphites, but the principal depôt of gryphites there lies in the ferriferous limestone at the eastern foot of the forest. In Sangerhausen and Bottendorf, small terebratulites. In Saalfeld, ammonites; and in Lower Saxony, the lower strata contain entrochites in great number, and also serpulites and bivalves.

Remark.—In those tracts occupied by the weissliegende, marl shale, and zechstein, may be observed all those varieties of position that are so common in coal fields; inflections, troughs, and saddles, with foldings of the strata, arising chiefly from the inequalities of the fundamental rock, but seemingly also in part from unequal pressure exerted in a lateral direction. To these are to be added alternate depressions and elevations of the strata, produced by slips or faults. The faults are true veins, usually occupied by calcareous spar, heavy spar, and quartz, with mineral pitch, and ores of copper, iron, cobalt, nickel, and galena; sometimes also they are filled with a conglomerate, composed of fragments of swinestone, zechstein, and shale. These veins penetrate occasionally below the weissliegende; very rarely, however, to any great depth into the old red sandstone.

Most of these beds acquire a greater thickness, in proportion to their increased descent into the earth.

Upper Portion.

Rauhwacke.—When this appears, it reposes always on zechstein, varying from 21 inches to six, seven, and more fathoms in thickness. It is a porous, siliceous, limestone, partly bituminous and cavernous: the cavities in it sometimes occurring three or four fathoms in breadth and height. Fetid, and of great variety of aspect and structure. Grey and black, of various shades; compact, or foliated granular, with dull spots and streaks; amygdaloidal, with round grains of white carbonate of lime; vesicular and scorious; brecciated, with a sandy or compact base; also granular and oolitic. The foliated granular variety is sometimes yellowish-white, and passes into sparry iron ore, or into ferriferous limestone.

Minerals occasionally intermixed: calcareous spar in grains, or in single folia; earthy and slaty aphrite, in large nests, in layers and strings, and disseminated; quartz in round portions, independent of the general diffusion of siliceous matter, and also in crystals, in nests or layers of iron ochre and sparry iron ore; brown ironstone, in lenticular nests or nodules; and, more rarely, iron pyrites in filamentous veins, or disseminated. No petrifactions have been observed in it.

Rauhstein is chiefly distinguished from rauhwacke by its greater simplicity of structure, and by containing only small drusy pores. Fetid, and effervesces violently with acids. Grey, with streaks of brown; partly friable, partly solid, as compounded of rough sandy particles, or of crystalline; texture, more or less distinctly slaty. Also brecciated; and amygdaloidal with grains of carbonate of lime and quartz. Contains sometimes rounded and angular portions of marl, blue clay marl, and nodules of rauhwacke; besides aphrite in grains, membranes, or flocculent. Rauhstein commonly reposes on rauhwacke or zechstein, being covered by asche or swinestone; but it is sometimes found in single masses or layers, imbedded in rauhwacke or in asche, and also in blue clay marl with nodules of gypsum. A considerable proportion of the rauhkalk of the forest of Thuringia appears to be the same substance.

Asche, or earthy swinestone, seems in a great measure confined to Mansfeld and the adjoining parts of Thuringia. It is composed of fine sandy or pulverulent particles, which in its native seat are compacted to a certain degree; but on exposure become friable, and fall into a fine dust on the slightest touch. Very fetid, and effervesces violently with acids.* It passes on the one hand into black clay marl, and on the other into swinestone. Minute dusty particles of mica and calcareous spar are usually intermixed with it; also spots and streaks of iron ochre. Aphrite, earthy, foliated, and slaty, occurs in it in large round masses, exceeding the head in size, or in numerous layers from one to six inches in thickness, also disseminated. Beds of clay marl, and thin layers of quartz sand, appear also in it. The asche commonly forms a bed, from three to ten feet thick, lying between rauhwacke and swinestone, but it is sometimes three, four, or eight fathoms in thickness, yet then never pure. Sometimes also it lies between zechstein and rauhwacke; and, more rarely, it appears in slight beds, alternating with gypsum and swinestone. When covered by gypsum, it contains spheroidal masses from one to one and a half foot in diameter, composed of concentric alternating layers of gypsum, swinestone, rauhstein, or asche, with disseminated aphrite. It is free from petrifactions.

Swinestone.—This substance occurs in different states: in pure strata; as a conglomerate; or in union with gypsum. The last will be considered under the head of gypsum.

The stratified swinestone is found varying in thickness from three feet to 20 fathoms. Brownish-black, or bluish-grey, with black cloudy spots; thin slaty, passing into compact, or imperfectly foliated. Dendritic delineations are common in it. Stratification seldom permanently regular, being subject to undulated, or sudden angular, inflexions; the strata sometimes forming also circular concentric groups. Contains incidentally nodular masses of drusy sandstone, and also cavities, a few occur in the greatest variety as to form, magnitude, and connection with each other; but they all bear evidence of owing their origin to the continued action of subterranean currents of water. They usually form a connected series, extending several miles under the earth, and filled with water to a certain level, which either flows off from stage to stage to lower caverns, or finds an outlet at the surface, constituting in some places lakes, or pools of water. A remarkable connected chain of such caverns was discovered near Wimmelburg, a few years since, by mining operations, at the depth of 50 fathoms from the surface, proceeding as far as explored 2100 feet upon the line of range, but probably to a much greater distance, if we may judge by the fractures and sinkings of the earth, which appear at the surface. The most spacious of these caverns are from 100 to 125 feet wide, and from 70 to 84 feet high.

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* In composition and general character, asche seems somewhat analogous to the *rottenstone* of Derbyshire. The geological position of the latter, however, is very different, being found in the first floetz or carboniferous limestone.

These disruptions and sinkings of the strata, which mark the course of the lower gypsum, differ much in size and form, resembling a crater or vertical hole with abrupt naked walls, a circular depression with sloping sides, or a long drawn winding concavity in the form of a valley. Hollows of this description are found empty, or filled with water, fresh or salt, and constantly or periodically so. On the other hand, the sinkings which accompany also the upper gypsum in the new red sandstone formation, occupy a more extended space, with easier slopes and gentle routlines; the difference of which is to be found in the same action which operated on both formations, having been in the one case direct, and in the other mediate.

Salt springs usually accompany the cavernous gypsum; and in that of Bottendorf, examples are not wanting, although rare, of the occurrence of rocksalt in it, in the form of small massive pieces, or in thin veins, extending to the breadth of two fingers.

Equivalents or Substitutes.

As equivalents or substitutes of the preceding members of the limestone formation, may be remarked in some tracts, a cavernous limestone, and a ferriferous limestone.

The *cavernous limestone* (Höhlen-kalk) seems in general character to be most nearly related to rauhwacke, being also vesicular, and distinguished in like manner by great fissures and dislocations of the strata. In the Hartz and the forest of Thuringia, it is known by the name of *rauh-kalk*, where, though of local occurrence, it extends over considerable districts, forming in some parts cliffs of a grotesque appearance. In the former country, it is described by M. Jordan as a yellowish-grey, slightly fetid, limestone; compact or granularly foliated, rough to the touch, with numerous cavities, partly occupied by calcareous spar; and free from organic remains. It prevails in the southern and western parts of the Hartz, being in some parts from 14 to 24 fathoms thick, and containing numerous caverns, celebrated for the remains of extinct species of the bear, and other quadrupeds, found in them. The Scharzfeld cavern extends about 350 feet in length.

The rauhkalk of the forest of Thuringia lies sometimes above swinestone. According to M. Heim, it is found in two states; as a loose friable marly earth, employed in manuring sandy soil; or as a grey compact rock, with intermingled calcareous spar, distributed in every direction; and destitute of organic remains. It is tough and firm, and generally porous, cellular, or cavernous, containing cavities, from the size of the fist to that of the body of a man, which are mostly filled with dusty marl. This is its usual state. But by M. von Hoff, it is described rather as a crystalline granular limestone, more or less porous and rifted, and grey, of every shade, but particularly yellowish-grey; containing frequently ochry spots and ferruginous particles, and traversed by numerous veins of calcareous spar of variable thickness, the lowest strata passing into swinestone. It sometimes contains pectinites and beautiful corallites.* The rauhkalk of the Forest frequently occurs in large shapeless masses, without the smallest trace of a stratified structure; and when distinct strata do appear, they are often in great disorder from disruptions and sinkings of the earth, produced by the enormous fissures that are so characteristic of that rock. These fissures are mostly vertical, and either empty, or partially filled with stalactite or clay; and being occasionally expanded in their course, they form in some places extensive caverns, some of which contain bones. It is stated by M. Heim, that this limestone obtains in the forest of Thuringia a thickness of 70 to 120 fathoms, and if the subordinate beds of the formation be also included, a thickness of 200 fathoms.

The rauhkalk of Leitnitz, near Blankenburg, is traversed in every direction by veins of lamellar heavy spar, from 2 to 16 inches thick, and by slight strings of the same substance, which ramify and disappear. It contains heavy spar also in the form of beds and layers, as well as in scattered portions, both massive and disseminated.

Near Weyda, between Gera and Neustadt, the lowest beds of the formation, which there repose immediately on greywacke and greywacke slate, consist of a siliceous drusy limestone, alternating with thin layers of sandy clay marl, and bearing nests and thin layers of copper ores, which form in the aggregate a thickness of six, or eight, and sometimes even 30 inches.

Ferriferous Limestone (Eisenkalk).—This is found more particularly at the southern, south-eastern, and eastern foot of the forest of Thuringia; while in the Hartz, it appears to be of rare occurrence.

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* Baron von Schlotheim has observed in the cavernous limestone, near Glücksbrunn and Liebenstein, keratophytes dubius, k. anceps, and escharitis retiformis; also gryphites aculeatus, g. speluncarius, tellinites dubius, mytilites ceratophagus, m. striatus, terebratulites cristatus, t. pelargonatus, t. sufflatus, t. elongatus, trochilites helicinus, encrinites ramosus, and trilobites problematicus. (See Petrefactenkunde.)

According to M. Voigt, when the limestone mass becomes less cavernous and more ferriferous, it appears as a yellowish-brown, or brownish-grey, compact rock, firm and hard, or tender and earthy. It occupies the place of rauhwacke and swinestone, and it contains beds of swinestone, and sometimes also of bituminous marl shale, and being distinguished by incidental strata containing gryphites, M. Voigt has designated it by the name of *gryphite limestone*.* On the other hand, M. Heim describes it, when porous, as rauhwacke, differing only in respect of the manganese and iron contained in it. That variety called *zuchtwand*, a calcareous ironstone, appears also to come under this head, being near the surface yellow, in the interior brown, and at a greater depth black, and passing in some parts into sparry iron ore. It contains numberless nests and strings of calcareous spar, and, in the vicinity of veins, also nests and strings of heavy spar. And near Schmalkalden, it appears to contain also purer beds of ironstone, constituting there the lower part of the formation, from 17 to 30 fathoms thick, and reposing immediately on primary rock. The calcareous ironstone is there connected with every variety of brown ironstone, and, in the vicinity of veins, also with occasional nests of sparry iron ore, and lamellar heavy spar.

According to M. von Hoff, the ferriferous limestone, when examined by the lens, is found to consist of small foliated grains surrounded by an ochry earth, passing from yellow into dark-brown. It is characterized by numerous veins of calcareous spar, sparry iron ore, and manganese, which last substance appears also in dendritic spots. At a greater depth, the colour becomes darker, the predominance of iron greater, and at length a subordinate bed of *brown ironstone* appears, consisting of every variety of this substance, together with sparry iron ore, manganese, and lamellar heavy spar, occasionally intermingled with copper ores; and thus constituted, the formation reposes on the fundamental rock. Distinct deposits of copper ores, accompanied with some manganese, appear also in the ferriferous limestone, particularly when reposing on granite.

In the vicinity of Gera, the beds of swinestone, black limestone, and ferriferous limestone, which occur there, contain gryphites, as well as those needle-like petrifactions, which were considered as their spines even by Walch. The ferriferous limestone contains also pectinites.

The relations of the *brown ironstone* bed may be observed in a distinct manner near Camsdorf, and in other parts of the Circle of Neustadt. At the former place, it constitutes a bed, from three feet to six fathoms thick, extending through the whole district, with occasional interruptions in spaces of 60 to 170 fathoms; consisting of limestone intermixed with brown ironstone, sparry iron ore, and lamellar heavy spar, and containing large drusy cavities, in which the heavy spar sometimes appears in magnificent tabular crystals, one foot square. The bed reposes upon bituminous marl shale, and is commonly covered by a bed composed of an intimate mixture of limestone and fine granular sparry iron ore, with drusy cavities, containing crystallized arragonite. But beneath the bituminous marl shale, sometimes appears a second ironstone bed.

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* The following organic remains have been observed by Baron von Schlotheim in gryphite limestone: gryphites aculeatus, g. cymbium, g. gigas, probably belonging to gryphæa arcuata of Lamark, terebratulites alatus, t. lacunosus, pectinites textorius, tellinites sanguinolarius. (See Petrefactenkunde.)

An ironstone bed occurs also below bituminous marl shale in the gryphite limestone, near Bennowitz, Königsee, and Blankenburg.

These ironstone beds are sometimes accompanied by more or less considerable deposits of copper ores.

The preceding abstract will convey to the reader a general idea of the relations of the lower limestone formation of M. Freiesleben; but to elucidate more fully the peculiarities displayed by that formation in different tracts, the following remarks are subjoined.

In the Hartz, the copper shale is much richer in metal on the southern than on the northern side of that country; in the latter quarter resembling rather a bad coal. In the forest of Thuringia also it is metalliferous only in particular quarters; while in others it forms several beds, which, in some cases, exhibit a geological affinity, as it were, to the beds of coal which lie not far beneath it.

On the western and north-western side of the Hartz, the zechstein forms, according to M. Haussmann, considerable ranges, spreading far into Lower Saxony; but at the south-western side it is partly accompanied, and partly replaced by rauhkalk, which there always occupies the place of rauhwacke; and the same rock is widely spread also over zechstein on the north-western side of the Hartz.

In Riegelsdorf, in Hesse, several thick beds of porous fetid limestone alternate with equally thick beds of reddish and bluish clay (the latter sometimes containing gypsum), and with beds of compact gypsum and swinestone; the whole reposing on zechstein.

On the north-western side of the forest of Thuringia, the limestone formation reposes, partly on old red sandstone (rothe todtliegende), partly on granite or other primary rocks. In the former case, its stratification is pretty regular; in the latter, it is irregular and interrupted, which is also the case on all the other sides of the forest, the formation frequently appearing there in detached and fractured portions. The upper beds have the purer form of compact limestone; the lower consist rather of swinestone, rauhwacke, and zechstein, among which, however, rauhkalk sometimes appears. When in contact with primary rocks, the lowest bed is composed of the calcareous ironstone, noticed above*, accompanied with sparry iron ore, and heavy spar, and sometimes also with ores of copper. On the other hand, at the eastern and south-eastern foot of the forest, the formation consists of the bituminous ferriferous limestone, with beds of swinestone and bituminous marl shale.

M. Freiesleben, in tracing the lower limestone formation, extends his views also into the south of Germany, Dalmatia, the Pyrenees, &c.; on which occasion he notices the opinion of Baron A. von Humboldt, "that the zechstein of Thuringia is coeval with the floetz limestone of the high Alps;" and the representation of Baron von Buch, "that the colossal calcareous chain which forms the northern side of the Alps in Bavaria, Salzburg, Austria, and Stiria, belongs to the same era, containing within that tract the great deposit of gypsum and rocksalt."*

The limestone formation of Upper Silesia, containing, near Tarnowitz, galena, calamine, and ironstone, is also referred by Baron von Buch to the same period.†

If we now turn to the English magnesian limestone formation (which comprises also argillaceous and sandy limestone), as the equivalent of the German, just described, we may find several corresponding characters indicated in Mr. Winch's account of Durham and Northumberland (Geol. Trans. vol. iv), and in Mr. Farey's Description of Derbyshire (vol. i). For the general distribution of that formation (including the calcareous conglomerate), reference may be had to Mr. Greenough's Geological Map.

3. *Clay and Sandstone Formation of M. Freiesleben.*

Second Floetz Sandstone Formation of Werner.

New Red Sandstone Formation.

This passes by various gradations into the lower limestone formation, and apparently also into the upper or shell limestone. It is widely distributed, filling up those great basins or concavities that are enclosed by rocks of an earlier era, and appearing always to increase in thickness in proportion to its distance from the outcrop of the subjacent limestone formation. Its strata are found very differently disposed, and generally unconformable to the older formations; varying rapidly from the horizontal to the vertical position; also undulated and forming troughs. In the forest of Thuringia, it acquires a thickness of 1200 to 1500 feet, but in those parts of Mansfeld which adjoin the lower limestone formation, its thickness varies from 120 to 420 feet; while at a greater distance, it has been found 800 feet thick.

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* But it was reserved for Professor Buckland to establish this position in a clear light, as well as to solve other doubtful points affecting the same regions.

† Von Buch. Geog. Beob. p. 104, and 108—113 of vol. i.

The formation appears under different circumstances in different districts. In a great part of the forest of Thuringia, it consists of uniform sandstone, beneath which is found a bed of clay with gypsum and rocksalt; and above the sandstone occurs another bed of clay with interposed beds of marl, gypsum, rock-salt, and bituminous layers. M. Heim states each of these beds of clay to be 200 feet thick. In Mansfeld, on the other hand, the formation consists of four principal members, *variegated clay or clay marl, sandstone, slaty sandstone, and roestone*, beside other beds incidental to it, which will be noticed hereafter. But the principal members may be said to be inseparable, for where one occurs, the others also are generally to be found; and they pass by insensible gradations into each other, being commonly disposed in moderately thick beds, in indeterminate order. Yet when one or the other member (particularly the sandstone or clay) acquires an unusual thickness and predominates, the others are still not wholly wanting.

Principal Members.

Clay.—This, whether pure, or in the state of clay marl, is generally brownish-red, thin and straight slaty, glimmering, and tough, disposed in thin strata; or crumbly and unstratified, in great thickness. Commonly mixed with minute scales of mica, or with fine sand, passing into slaty sandstone; and sometimes it appears as indurated clay or claystone, with a flat conchoidal fracture, of a brownish-red, greenish-grey, or mountain-green colour. With the thick beds of red clay, as also with those of the roestone and sandstone, there frequently alternate thin layers of grey, yellow, green, and bluish clay. These also pass into slaty sandstone and argillaceous sandstone, by an admixture of mica and sand. The clay is commonly more or less calcareous, whatever its colour may be. No petrifactions have been found in it.

Sandstone.—This, which forms the most distinguished and predominant member of the whole formation, consists of several varieties, possessing an argillaceous, calcareous, or siliceous cement; but principally of those whose cement is of the nature of the first two; all of them, however, alternate with each other, and also in endless diversity with clay, slaty sandstone, roestone, and calcareous beds.

The *argillaceous sandstone* is white, yellow, or grey, brownish-red, or variegated. The first three kinds are fit for the purposes of the architect and statuary, consisting, for the greater part, of fine or minute grains of quartz, of an equal consistence, with an argillaceous cement, and some mica. Hence it has been called by some writers quadersandstein, or freestone.* The brownish-red, or reddish-brown variety is tender or friable, and liable to be affected by the vicissitudes of the atmosphere; or it is firm and darker coloured, perfectly resembling some varieties of the rothe todtliegende. The variegated sandstone is found fine grained, tender, and friable; also porous, or reddish-brown, with stripes of hair-brown, formed by grains of iron oxide, or of roestone; sometimes also the cement is partly calcareous, and the stone full of small cavities, filled with crystals of calcareous spar. This variety is connected with indurated sandy clay, containing nodules of marl, the interior of which is usually occupied by druses of calcareous spar.

These varieties of argillaceous sandstone are characterized by their planes of separation being coated with grey, white, and black mica; and by containing *clay galls;* namely, round or angular portions of clay, or slaty clay, mixed with mica, and of a grey, red, brown, green, or yellow colour. Nodules of yellow earth sometimes accompany them, and ferruginous points and circular spots are frequent, beside occasional layers composed of grains of ironstone, of the size of a lentil; likewise gæodes and balls of ironstone, and similar concretions of mica, indurated iron ochre, and ferruginous sandstone. The thicker and firmer beds of sandstone and roestone are frequently traversed by veins and strings of calcareous spar, which rarely exceed one, or at most a few inches in thickness.

Organic Remains.—M. Freiesleben notices the occurrence of an orthoceratite in the variegated sandstone of Nebra; and in referring to the work of M. von Schlotheim, which appeared in Leonhard's Taschenbuch, in the year 1813, he speaks of pectinites, pinnites, pholadites, turbinites, and large ostracites, as being principally characteristic of the variegated sandstone; but M. von Schlotheim himself observed, that so great an uncertainty has prevailed with respect to the different formations of sandstone, in which organic remains have been found, that continued investigation alone could determine how far the lists ascribed by him to the variegated sandstone, and quadersandstone, respectively, were correct. It now appears from the Petrefactenkunde of that author, published in 1820, that the shells noticed above occur in the quadersandstone, and not in the variegated sandstone. B. von Schlotheim mentions, however, having observed in the latter, gryphites spiratus, palmacites annulatus, carpolithes malvæformis, c. secalis.†

The *siliceous* or quartzy sandstone, composed of fine granular quartz, with a siliceous cement, occurs much less frequently in

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* The term *quadersandstein* is, however, now more peculiarly applied to the third floetz sandstone formation.

† The remains of organized bodies are, I apprehend, very rarely, if ever, met with in the new red sandstone of England.

Mansfeld, being imbedded, in no certain order, among the other beds of sandstone and clay. But in the forest of Thuringia, M. Heim observes, that it occupies the central portion of the strata, while the argillaceous or calcareous varieties are found toward the exterior, where they are confined by the clay and concomitant beds that intervene, on either side, between the sandstone and the lower and upper limestone formations.

The *calcareous* sandstone forms a substance, of a character intermediate between sandstone and roestone, usually appearing in beds from a half to one foot thick, the interior of which is commonly dark bluish-grey, passing toward the exterior into smoke-grey. The darker varieties consist of an intimate mixture of lime and silex, hard and firm, in which single grains of quartz are seldom discernible; the lighter striped varieties are distinctly sandy, or they have a small fine-grained sparry texture. It is seldom porous, but when in that state, it has little cement, consisting of minute white grains of quartz. The calcareous sandstone, or siliceous limestone, passes usually into roestone.

Roestone. (Roggenstein.)—This substance varies considerably in the size, form, and connexion of the grains; the structure of which is sometimes compact splintery, sometimes concentric lamellar, sometimes foliated granular, and commonly of a smoke or dark-grey, brownish-red, or reddish-brown colour. The grains are found from the size of cherrystones to that of peas, hemp, and millet seeds, and even much finer. The coarser grains are met with closely adhering to each other, with little appearance of a connecting medium, or immersed, in a more or less crowded state, in a base of highly indurated clay or marl, mostly of a grey or reddish colour; or they appear singly in a sandy or marly base. The grains have a rough, uneven, or a smooth surface; being in the last case usually white, and composed of concentric lamellar concretions. Another variety of the finer-grained consists of grey or brown, round or oval, grains, with a foliated granular texture, lodged in a milk-white base, of a similar structure.

As the grains become smaller and gradually diffused in the substance of the base, the hardness and tenacity of the stone are proportionably increased; so that at length the roestone passes sometimes into a coarse or fine splintery rock, a quartzy compact limestone, or *horn-marl.* And, generally speaking, the gradations are very numerous, by which it passes into sandstone, marl, compact limestone, hornstone, and even into sandstone. The gradation into sandstone occurs in particular in the finer-grained white, or brownish-red sandstone, which contains minute brown grains of roestone. Sometimes also both the roestone and calcareous sandstone pass into a substance nearly resembling sparry iron ore; and in the clay and sandstone quarries near Sangerhausen is a considerable bed of oolitic sparry iron ore, the grains of which are of the size of peas, with a hair o clove-brown colour, and foliated granular texture, the cement of which is a yellowish-grey, coarse sandy marl. In other quarters, the grains of roestone sometimes consist of brown ironstone, e. g. near Eisleben.*

The compact roestone is frequently marked with beautiful dendritic delineations.

The roestone usually occurs in beds of 4, 8, and 12 inches in thickness, which alternate with sandstone (particularly calcareous sandstone), slaty sandstone, and variegated slaty clay; it lies much more rarely between beds of red clay. Clay galls are also common in it.

Membranous malachite has been found in the roestone between Emselohe and Blankenheim; and in calcareous sandstone, near Sangerhausen. Traces of copper occur likewise in the variegated sandstone of the forest of Thuringia.†

It has been remarked by M. Voigt, that the roestone occurs seldom, if ever, in the new red sandstone, when the shell limestone formation is found to predominate in the same tract.‡

Slaty Sandstone.—This is in a great measure confined to those quarters in which thin beds of argillaceous sandstone, or of roestone, alternate with slaty clay, and it never appears in any great thickness. It is usually very micaceous, and of a tender consistence, passing into micaceous slaty clay, or into sandstone.

Incidental Members.

Limestone and Marl.—These substances are sometimes found in the new red sandstone formation, unconnected with roestone, commonly appearing in an isolated manner in the clay, in the vicinity of the upper gypsum. Of the limestone, several varieties occur, e. g. fine splintery and siliceous, sandy and tender, solid and minute granular, and compact. Of the marl: sandy, rough, and porous, with nodules of sandy limestone; or as slaty clay marl, with round portions of calcareous spar.

Loose Sand and Conglomerate.—Beds of these have been sunk through in several parts of the county of Mansfeld, and at first, it was doubted whether they might not be alluvial. But more extended observation has proved that they belong to the general series. The conglomerate commonly consists of red clay mixed with much sand, including more or less numerous pebbles and boulders of quartz, hornstone, lydianstone, granite, syenite, greenstone, and quartzy sandstone. The sand is either a pure loose running sand, or it is mixed with clay or marl, and loosely coherent; but both occasionally contain angular fragments of felspar, beside pebbles and grains of quartz, hornstone, granite, syenite, and slaty red sandstone. In the sand, a large well-preserved chamite has been found at the depth of 12 fathoms from the surface, beside fragments of shells. And petrified wood with pieces of bituminous and pyritous wood have occurred in loamy sand, somewhat indurated, at the depth of 46 fathoms.

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* Hence we have seen that in the *new red sandstone* formation, the oolitic structure has appeared in compact limestone, in foliated granular limestone, in sparry iron ore, and in brown ironstone; and in the subjacent or *lower limestone formation*, in swinestone, and rauhwacke.

The oolitic structure is known also to exist in the carboniferous or first floetz limestone; e. g. in the environs of Tortworth and Bristol; but it is in the upper portions of the shell limestone that it becomes predominant.

† Ores of copper and lead, and earthy black and brown cobalt ore, are found in the new red sandstone of England, at Alderly Edge, in Cheshire.

‡ This observation appears to hold good in England, where I am not aware that the roestone has ever been met with in the new red sandstone formation.

Ironstone.—This appears in Mansfeld in the form of pure ironstone, or as ferruginous sandstone. The former is a brown argillaceous ironstone, partly also very siliceous, and either compact or ochraceous, and free from petrifactions; forming layers from half an inch to several inches in thickness, or round masses and geodes, imbedded in the clay or argillaceous sandstone. The ferruginous sandstone constitutes yellow and brown layers, which are frequently very siliceous, and usually three or four inches thick, and sufficiently rich to be worth smelting. But in Sangerhausen, a bed occurs from two to six feet thick.

Upper Gypsum.—This was formerly considered as a distinct formation, the position of which was conceived to be between the new red sandstone and the shell limestone. Later writers have shown its geological connection with clay; and M. Heim views it in the forest of Thuringia as subordinate to the bed of clay, which there intervenes, on either side, between the new red sandstone and the lower and upper limestone formations.

But in Mansfeld and the adjacent districts, the upper gypsum has no such determinate position, being there found subordinate to the clay and sandstone formation, principally in the clay, but sometimes also in the sandstone, yet in no certain order. The former may be called clay gypsum, and the latter sand gypsum.

The upper gypsum is found in thick strata horizontally disposed, or variously inclined, according to the change of position it may have undergone, being not unfrequently traversed by irregular fissures, often several feet in width, which are either open, or filled with clay, sand, or gravel. The cause of the frequently disturbed position of the upper gypsum, and of the concavities by which the surface is marked, is easily to be found in the sinkings of the earth, produced by the rupture and depression of the cavernous formations below.

In Mansfeld and the adjoining districts, the clay gypsum occurs in the form of large, detached, cliff-like masses, enveloped in the red clay, sometimes constituting a chain of small hills, but never, as in Thuringia, regularly continuous beds. Its position is indeterminate; appearing in some quarters in the uppermost beds of clay, almost immediately under the shell limestone; in others, occupying an intermediate position, being covered by an alternating series of beds of sandstone, roestone, and clay, and reposing on a similar series; while in most places, it is found in the lowest beds, adjoining to the subjacent cavernous gypsum and limestone. But in all these cases, it forms isolated, shapeless, masses, lodged in the clay; the largest and most connected appearing in the lowest position, and the more divided and numerous in the intermediate and uppermost. Yet in those parts, where the gypsum masses are wanting, traces of this mineral are still to be met with, in the form of round nests, single grains, or thin streaks. The enveloping mass most frequently consists of the red, slaty, tenacious clay, pure, or fine sandy; it, however, often alternates with layers of sand, or tender sandstone; and in some quarters it contains boulders and pebbles of quartzy sandstone, granite, syenite, &c. of various sizes, and disposed in layers, e. g. near Wolferode and Wimmelburg.

The sand gypsum is to be observed near Grosleinungen, in connected masses, forming a continuous chain of cliffs, where it reposes on an alternating series of beds of calcareous sandstone, indurated marl, limestone, and sandstone; and which separate there the sand gypsum from the subjacent swinestone and lower gypsum. This sand gypsum contains frequently layers and streaks of red clay, large round masses of firm sandstone, and lamellar portions of slaty micaceous sandstone.

The principal constituents of the upper gypsum are the foliated and radiated kinds, with a smaller proportion of the specular, fibrous, and earthy: being found grey, white, bluish, or reddish. In the clay gypsum are also found occasionally carbonate of lime in spots and stripes, and small rock crystals in prisms with both the pyramidal terminations, e. g. near Wimmelburg. Native sulphur has likewise occurred in it near Naundorf.

In Mansfeld, the thickness of the upper gypsum has been found varying, in different places, from 2 to 30 fathoms.

Rocksalt.—Several geologists are of opinion that saltsprings derive their source from the clay and gypsum of the new red sandstone formation. It does not appear that salt springs always proceed directly from a body of rocksalt, or from salt connected with the gypsum, but frequently rather from saline particles diffused through the clay. Hence the clay, as well as the gypsum, may be said to be saliferous. This view of the case is confirmed by experience.

At Schombeck, in Magdeburg, salt springs rise from a bed of clay, immediately below the shell limestone; and at Aschersleben, near Halberstadt, a salt spring rises either from shell limestone, or from the clay of the new red sandstone formation; while at Salzhennersdorf, in Hanover, salt springs proceed from the upper gypsum. In the forest of Thuringia, salt springs issue from the bed of clay that lies below the new red sandstone, near Salzungen and Schmalkalden. Near Sulz on the Ilm, Lindenau in Hildburgshausen, and Kreuzburg in Eisenach, they proceed from the upper bed of clay that lies between the sandstone and the shell limestone. The salt springs near Sulz der Helden and Sulben, in Lower Saxony, rise also, it is said, from the new red sandstone formation. According to M. Schulze, most of the salt springs with which he is acquainted rise from, or in the vicinity of, the shell limestone; but their true source is conceived to lie in the red clay marl, from which they have forced their way through the shell limestone.

Coal.—In Mansfeld, there is no appearance of coal in this formation. But in the forest of Thuringia, traces of coal occur, according to M. Heim, in the bed of clay, which is interposed between the new red sandstone and the shell limestone, both on the Franconian and Thuringian sides of the forest; forming there thin discontinuous layers, scarcely one inch thick, separately considered, but which sometimes coalesce, and thus constitute a bed from one to two feet thick. These layers consist of black shale, with thin streaks of coal, accompanied with iron pyrites, gypsum, and calcareous spar.

* * *

In taking a general view of the distribution and extent of the new red sandstone formation in Germany, M. Freiesleben traces it from Mansfeld into Thuringia, the Hartz, Lower Saxony, Westphalia, Hesse, forest of Thuringia, Franconia, Suabia, Bavaria, and Austria, availing himself on this occasion of the researches of MM. Heim, Jordan, Voigt, Hausmann, Flurl, &c.

* * *

The great extent and distribution of the new red sandstone tracts in England may be seen in Mr. Greenough's Geological Map. It is well-known that the principal English deposits of gypsum and rocksalt are found in that formation, as well as the sources of salt springs. And from the detailed observations of Dr. Holland on the Cheshire salt district,* of Mr. Horner on the Droitwich brine springs,† of Mr. Winch on the new red sandstone of Durham,‡ and of Prof. Buckland on that of Cumberland and Westmoreland,§ beside other publications, we may collect many facts coincident with what have been remarked in the German formations. On the banks of the Teas, the new red sandstone formation is said to be at least 720 feet thick.

(*To be continued.*)

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* Geol. Trans. vol. i.

† Ibid. vol. ii.

‡ Ibid. vol. iv.

§ Ibid. vol. iv.

ERRATA.

Page 341, line 27, *for* its thickness is variable, *read* its thickness is very variable.

344, the two lines placed above the words "*Upper Portion*" should have been beneath them.

348, line 24, *for* gentle routlines, *read* gentler outlines.

244, (October Number) line 15, *for* Group No. 21, *read* Group No. 20.

Article IV.

On the primary Form of the Bournonite, as deduced from its Cleavage and Planes of Modification. By William Phillips, FLS. &c.

(To the Editor of the *Annals of Philosophy.*)

In the Transactions of the Royal Society for 1804 is a communication by the Comte de Bournon on this substance, with a plate of 17 of its crystalline forms. In the volume for 1808 there is a paper by James Smithson, FRS. on the forms of the crystals as they occur in nature, and on the primary form as deduced by him from the planes of modification to which it appeared to him liable. The strictures of the latter gentleman induced the Comte de Bournon to re-examine the Bournonite, as to its primary crystal and modifying planes, and an elaborate article on the subject is inserted in Nicholson's Journal.

The Comte de Bournon, in the first instance, was induced to assume the *rectangular tetrahedral prism* as the primary form. Mr. Smithson substitutes the *cube*, asserting at the same time, though not in very courtly phrase, that "of the 17 figures given by the Comte de Bournon, great part are acknowledged to have no existence, nor indeed are any of them consistent with nature;" a remark which doubtless originated, in part, in the want of accuracy in the drawings, but chiefly in the errors of Mr. Smithson's own admeasurements.* The Comte de Bournon, in Nicholson's Journal, and afterwards in his own "Catalogue," substitutes a *square prism* as the primary form, and gives a more detailed series of the crystalline forms of this substance.

The last determination of the Comte de Bournon appears rather to have receded from, than approximated to the truth, though at least as near to it as the cube adopted by his opponent.

It does not seem to have been known to either of these gentlemen, that this substance possesses regular cleavages. It is divisible into two prisms; namely, a rectangular prism, and a rhombic prism, owing to its possessing two sets of cleavages—a circumstance not very uncommon among minerals.

In my collection are several specimens of the Bournonite from Huel Boys, in Cornwall, and a considerable number of isolated crystals; one of them near an inch in length, and half an inch in breadth and thickness. From 16 of these crystals I have taken about 200 measurements, by means of the reflective goniometer; and these, together with the cleavages I have observed, enable me, as I trust, to put it beyond doubt, that the form of the primary crystal is neither the square prism, nor the cube.

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* Of the eight angles given by this author, only two are correct: it may, therefore, be taken for granted that what he has given are not actual measurements, especially since they are given to a second, which no goniometer will indicate, but rather assumptions founded on the known laws of decrement belonging to the cube, which he erroneously assumed as the primary form.

The annexed figure (Plate XI), fig. 15, represents one of the most simple crystals: others exhibit several other planes, which, however, are not important to the present question—the form of the primary crystal.

This mineral may be cleaved with brilliant surfaces parallel to the lateral planes $P\ P''$, and $c\ c'$; the latter are generally striated in a direction perpendicular to the plane P': the former meet at an angle of 90° by the reflective goniometer, though sometimes varying a few minutes; the latter at about 93° 30′ and 86° 30′; I say *about* 93° 30′, because, owing to the extreme brittleness of the substance, which readily yields even to the pressure of the nail, it is difficult to obtain two perfect planes of cleavage on the same specimen, and even when attained, they rarely agree to a few minutes, owing either to the injury which the mineral sustains internally by cleaving it, or, as is frequently the case in the larger crystals, to their being composed of several smaller ones,[*] bounded in common by brilliant surfaces. I have not been able to discover a cleavage parallel to the terminal plane P'.

It is known that the natural planes even of small crystals, which, being the most perfect, are best adapted to the use of the reflective goniometer, do not commonly give coinciding measurements, but are subject to variations from 1 to about 40 minutes. Having submitted the annexed drawing, and the following measurements, to my friend H. J. Brooke, Esq. he observes, that 'the inclination of the planes c on c' being 93° 30′, and there being cleavages parallel to the planes $c\ c'$ and $P\ P''$, the primary form may be either a rectangular or a right rhombic prism, the plane P' being assumed as the terminal plane. As the planes $c\ c'$ are mostly striated, the *rectangular prism* $P\ P''$ may be chosen to represent the primary form.

'From the angle of 93° 30′ which the planes $c\ c'$ make with each other, the terminal edges of the rectangular prism will be to each other as 160 to 170; and assuming the angle P' on a to be 136° 16′, and to be the result of a decrement by one row on the greater terminal edge of the prism, its height would be to its greater terminal edge as 153 to 170; or, the three dimensions would be nearly as 15, 16, and 17.

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[*] It is to this circumstance of crystals being composed of several others, or of their being in part constituted by after-deposition, that we may occasionally attribute some of the planes attainable by fracture. We seem to cleave the mineral, when, in reality, we only separate two crystals, affording tolerably brilliant planes, but without that perfection which belongs to those of actual cleavage; these are, therefore, readily detected by the experienced eye, and may be termed *planes of composition*. Such are the fractures sometimes attainable parallel to the lesser diagonal of the primary prism of sulphate of barytes, and the planes that may sometimes be procured by taking off the edges of a cube of fluor.

'That the prism is rectangular, and not square, cannot admit of a doubt; but its height compared with its terminal edges may require to be altered, in investigating the laws of decrement, by which other secondary planes are produced.'

P′ on P or P″	90°	
P′ on *c* or *c*′.	90	
P on P″	90	
c on *c*′	93	30′
P′ on *d* or *d*′	147	30
P′ on *a*	136	16
P′ on *b*	136	48
P′ on *c* or *c*′	136	45
P on *c*	133	15

WILLIAM PHILLIPS.

ARTICLE V.

Astronomical Observations, 1821.

By Col. Beaufoy, FRS.

Bushey Heath, near Stanmore.

Latitude 51° 37′ 44·3″ North. Longitude West in time 1′ 20·93″.

Sept. 24.	Immersion of Jupiter's first satellite...		15^h	56′	55″	Mean Time at Bushey.
			15	58	16	Mean Time at Greenwich.
Oct. 7.	Immersion of Jupiter's second satellite		10	39	16	Mean Time at Bushey.
			10	40	37	Mean Time at Greenwich.
Oct. 8.	Occultation of λ Aquarius by the moon...	Immer.	5	31	00·6	Mean Time at Bushey.
		Emer.	6	27	51·8	

Oct.* 13. Occultation of Atlas Pleiadum by the moon, Immersion 9^h 2′ 17″; Emersion 9^h 59′ 11″; Mean Time at Bushey.

Occultation of Pleione Pleiadum by the moon, Immersion 9^h 2′ 56″: Emersion 9^h 49′ 41″; Mean Time at Bushey.

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* Not having had a map of the Pleiades before me, I may have mistaken the names.

ARTICLE VI.

Tables of Temperature, and a Mathematical Development of the Causes and Laws of the Phænomena which have been adduced in Support of the Hypotheses of "Calorific Capacity, Latent Heat," &c. By John Herapath, Esq.

(*Continued from* p. 274.)

Theory of Evaporation.

If the parts of the particles of a fluid have a degree of adaptation which will not allow them to decompose but beyond a certain temperature, the body, as soon as it has attained this temperature, begins to experience a decomposition in some of its particles. This decomposition must always take place in the superficial particles, because, from what I have already shown, it cannot in the interior, so as to produce an evaporation from that decomposition, unless at the term of ebullition. As soon as this decomposition takes place, the temperature in the neighbourhood falls, and the parts of the fluid in this place becoming specifically heavier, sink. A current by this means generally ensues, the cooled parts descending, and the warm rising. The higher the temperature, the more rapid the currents, and the greater the evaporation will evidently be. At length when the body reaches the temperature of ebullition, the rapidity of the currents and of the superficial decomposition are not sufficient to counterbalance the rapid accessions of temperature the body is receiving, and hence ebullition, or a violent decomposition in the interior of the body, ensues. Pressure, as I have shown, will influence the temperature at which ebullition takes place; but the pressure of gases, it will appear in a future proposition, has nothing to do with the absolute celerity of evaporation. Even the vertical currents I have been speaking of, though in high temperatures near the terms of ebullition they will always exist, are not essential to evaporation. In low temperatures where the evaporation is small, simple communication of temperature from the under strata of the fluid may be sufficient to keep up the temperature of decomposition without any current; and hence there may be a perpetual evaporation even from solids, as ice, &c. which we know to be the case by experience.

Were fluids composed of particles of uniform adaptation, but different in different fluids, their evaporations by this theory would have all the same law and relation to their respective temperatures of ebullition under equal compressions, which experiments seem to confirm. But at present my object is to treat of the laws and phænomena of the evaporation of water only, which indeed I have not near time to consider as they deserve, and, therefore, fear many beautiful phænomena connected with them I shall be obliged to leave untouched.

By his theory, it appears that ebullition and evaporation are distinct and separate phænomena, both indeed depending on the same cause, temperature, or the violence of collision, but the one influenced by a circumstance, namely superincumbent pressure, which has no effect on the other. For want of attention to this distinction some writers, and among them the Rev. Dr. Wollaston, have run into considerable errors. The Doctor, following the hints and example of the Spanish philosopher Betancourt, has endeavoured to contrive, by the temperatures of ebullition, and a thermometer which he terms a barometrical thermometer, a method of determining the heights of elevations. A very important consideration, however, having been overlooked by the latter, and not noticed by the former, renders the method, as it is now employed, scientifically incorrect. But I shall have presently to speak of this subject. My object is now to develop from physical considerations the mathematical laws of evaporation, that we may see by their accordance with phænomena whether the simple operations of nature, rationally investigated, will not enable us to discover and establish something in the shape of a probable and a consistent theory.

Definitions.

In consequence of the new and enlarged views I have taken, I have found it necessary to make an alteration in the term *numeratom* I have hitherto employed, and to introduce some new ones. I have already expressed my aversion to making innovations, and certainly disapprove of the introduction of new terms where it can be avoided; but in the present instance where, by pursuing the route of Newton, I have departed so far from the beaten track, I cannot see how I can conveniently do without it. However, I have been as sparing as possible; and though I employ the following terms, I shall be very ready to change them for any which may be thought more suitable or proper. Some of those defined I may, perhaps, not want in the course of my present inquiries; but I thought it better to introduce them at once on account of their analogy and affinity to the others.

Def. 1.—Numeratomum, derived from *numerus*, a number, and the genitive plural *atomorum*, of atoms, I intend to signify the number of atoms in a specified space, without regard to the equality of magnitude in the atoms, or uniformity of figure.

Def. 2.—Arithmeridone, derived from ἀριθμος, number, and the genitive plural μεριδων, of particles, signifies also the number of particles in a specified space, without any regard to their equality or similarity.

Def. 3.—Voluminatom, from *voluminis*, of volume, and *atomi*, atoms, denotes the number of atoms in a unity of volume.

Def. 4.—Ponderatom, from *ponderis*, of weight, and *atomi*, atoms, implies the number of atoms in a unity of weight, or the number of atoms whose aggregate mass of matter is unity.

Def. 5.—Megethmerin, from μεγεθος, of volume or magnitude, and μερις, a particle, denotes the number of particles in a unity of volume.

Def. 6.—Baromerin, from βαρος, of weight, and μερις, a particle, signifies the number of particles in a unity of weight, or the number of particles whose collective mass of matter is unity.

Def. 7.—When in the following part of this paper I speak of the megethmerin of a vapour, I mean the number of vaporous particles in a unity of volume, not taking into account how many or how few, nor whether there be, or be not, any gaseous particles mixed with them.

PROP. VII. THEOR. V.

If two portions of the same fluid, having equal depths, be equally and similarly exposed at the same temperature, the quantities evaporated will be directly proportional to the areas of the exposed surfaces.

For since the depths and temperatures are equal in both, no irregularity can arise from any inequality on either of these accounts; and since the fluids are the same, and equally and similarly exposed, there can be no inequality in the momentary evaporating action on like parts of the superficies; therefore, the evaporating influence, to whatever cause it may be owing, being the same on equal parts, the ratio of the whole evaporating actions, and, consequently, the ratio of the whole quantities evaporated in any small particle of time will be equal to the ratio of the evaporating superficies. But if throughout any small portion of time the evaporating causes on equal superficies be equal, they will be equal throughout the next portion, and so on *ad infinitum*: and, therefore, the whole quantities evaporated for any length of time indefinitely, will have the same ratio; that is, the ratio of the superficial arcs.

Cor.—This proposition is mathematically true only when other things being alike, the depths are equal. But if the depths are unequal, but so great that the evaporations for any given time will not sensibly affect the general temperatures of the fluids, the ratio of the quantities evaporated will still have very nearly the same ratio as the evaporating superficies.

PROP. VIII. THEOR. VI.

If any portions of the same fluid be cooled by evaporation alone, from any common temperature to any other common temperature, the quantities of fluid lost by evaporation will be directly proportional to the quantities of the liquid; and conversely, if any two portions of the same fluid lose quantities proportional to their weights by evaporation, these losses, if their temperatures were first equal will equally diminish their temperatures, if no extraneous temperature interfere.

Were the two portions similarly exposed at an equal depth, the proposition would coincide with the preceding, and the evaporations would be as the surfaces; that is, as the quantities of the fluid; and the temperatures being once equal will always be equal. The converse case under these circumstances is evident.

Again: because the fluids are the same, the decomposition in each particle at a given temperature is the same, and hence equally affects the temperature of that particle, or an equal portion of particles. Therefore if the decomposition or evaporation of p particles produce on a certain quantity Q of the fluid, a given diminution of temperature, the decomposition of $n\ p$ particles or n times that number will produce the same diminution on n Q, or n times the former quantity of the fluid. That is, the original and resulting temperatures of the fluid being equal, the quantities evaporated will have the same ratio as the portions of the fluid from which the evaporations are made, provided nothing extraneous affects the temperatures.

And since equal diminutions of temperature are accompanied with evaporations proportional to the quantities of the fluid, it follows conversely, that evaporations proportional to the quantities of the fluid produce equal diminutions of temperature.

Cor.—Hence if the decomposition be similar in each particle, the loss of temperature arising from it will be proportional, and, therefore, by knowing the diminution of temperature due to the decomposition of any quantity of the fluid, which we shall hereafter show how to compute, and by knowing the deficiency of weight in the fluid, we may easily determine the loss of temperature arising from the evaporation; and hence also the loss or acquisition due to any other cause we wish to examine.

Scholium.

Philosophers, by some means, seem very much to have neglected, if not to have entirely overlooked this theorem. Having made their experiments in cases where the temperature of the atmosphere or other circumstances have overbalanced the influence of the inequality of depth, they have found that the exposed area has by far the greater influence, and hence have considered the depth and quantity of the fluid as having little or nothing to do with the evaporation. Thus, to give a familiar example, which has often been submitted to calculation as a proof that the quantity of evaporated water is proportional to the exposed surface, let us take two vessels filled with water, of the temperature of the atmosphere, the one deep, and the other shallow; but both having equal mouths. Then if they are both equally exposed to the atmosphere, the quantities of water evaporated in any given time will be very nearly the same in each, and seem to bear no proportion to the masses of the water, which, perhaps, may be five or ten times as great in one vessel as in the other. Now if we conceive the evaporation to commence in each fluid at the same temperature and from equal superficial portions, it is plain, all other things being the same, that the first increment of evaporation will be equal in each. And the same will likewise be the case with the second and successive increments, because the diminution of temperature by the evaporation being generally much less than that communicated by the surrounding air, the loss is immediately supplied, and the two portions of water kept at precisely, or very nearly, the same temperature; and, consequently, their evaporations equally supported.

The same will also hold good, as I have mentioned in the Cor. to the preceding Prop. if the depths be ever so unequal, but too great to have the temperatures sensibly affected by the evaporations; for if the temperatures be constantly equal, and the surfaces equal, the evaporations must be equal, whatever the depths may be.

By such experiments as these philosophers appear to have been much deceived, and to have formed very erroneous ideas of the laws and effects of evaporation uninfluenced by other circumstances. Such phænomena are undoubtedly a decided proof that evaporation takes place at the surface, and not in the interior of the body; but to conclude from this, that the mass of the fluid under all circumstances has nothing to do with evaporation is quite a paralogism. Let us, for instance, imagine two unequal portions of the same fluid, exposing at the same temperature, equal parts of their superficies to equal and similar actions of the atmosphere. Then, because the temperatures, exposed surfaces, and atmospheric actions, are equal, the first increments of evaporation must likewise be equal. To carry on the same idea, let us, therefore, conceive that successive contemporaneous increments of evaporation for half a given portion of time continue also to be equal. Then, since equal evaporations would produce equal diminutions of temperature in equal masses of the fluid, in unequal masses they would produce unequal diminutions; and the temperature of the less mass would be much more diminished than the temperature of the greater mass. But at a less temperature there is, *cæteris paribus*, a less evaporation. Therefore, for the other moiety of the time, the evaporation of the less portion will be less than that of the other; so that the two parts of time being considered together, the whole evaporation in a given time will be less from the less mass than from the other. This, however, is only to be considered as true when the effects of evaporation are not accelerated or retarded, or at least not effectually counterbalanced, by any foreign interference. Even should that foreign interference counteract, but not be sufficient to overcome entirely, the effects of evaporation on the temperature, it will even then be found that the greater quantity, though it has the same evaporation superficies, will evaporate more in a given time than the less. This has actually been observed to be the case; for when equal surfaces of water are exposed under the same circumstances in vessels of unequal depths, that in the deeper vessel usually loses in a certain time more water by evaporation than the other.

Prop. IX. Theor. VII.

If any two portions of the same vapour be confined in vacuo over equal condensing surfaces, I say that the increments of condensation, or the little quantities condensed in any small particle of time, the temperatures being the same, are as the elasticities of the vapour.

I do not here inquire into the particular manner in which the particles of the vapour strike those of the fluid to condense; but the means of condensation, that is, the temperature at the times of the collisions being the same, the probability of condensation, or of striking in the particular manner to condense in the same particle, is evidently as the number of times of its striking the condensing surface in a given time; and in two systems of particles, the probabilities of condensation are as the numbers that strike in the same time. But it is plain these probabilities must, considered generally, be proportional to the condensations. Therefore, the condensations in any small particle of time, from two portions of the same vapour, the temperatures being the same, are proportional to the numbers of the particles which come in contact with the condensing surface during that time.

This being granted, let us imagine the two vaporous media divided into strata parallel to the condensing surfaces, in such a manner that if the particles in each strata were uniformly distributed throughout their respective spaces, the corresponding strata in each should be one, two, or the same number of particles thick. Then since the particles of the media and also of the condensing fluids are respectively equal and similar, and the only difference in the media is in point of megethmerin, it is the same as if one medium was dilated or compressed until its megethmerin be equal to that of the other; and, consequently, the paths of corresponding particles are similar in both. Hence, therefore, the numbers of times the condensing surfaces are struck in a given time by the particles of the first strata, are in a ratio compounded of the ratio of the arithmeridones and the ratio of the number of returns of corresponding particles in the two strata. But these numbers of times are as the probabilities of condensation, and, therefore, as the increments of condensation from the given strata. The strata being the same number of particles thick, and the condensing superficies being equal, the ratio of the arithmeridones is equal to that of the duplisubtriplicate of the megethmerins. And because the velocities are equal, the ratio of the number of returns will be equal to the inverse of that of the lengths of the paths, or to the subtriplicate ratio of the megethmerins. Therefore, the ratio of the increments of condensation from the first strata is equal to that compounded of the duplisubtriplicate and the subtriplicate of the megethmerins, that is, it is equal to the simple ratio of the megethmerins. But by the laws of gases demonstrated in my last paper, the ratio of the megethmerins, the temperatures being equal, is equal to the ratio of the elasticities; therefore, the condensations from the first strata have a ratio equal to that of the elasticities of the vapours. And by a similar train of reasoning, we might show the same thing to hold good in the second and higher strata; for the particles of the second and superior strata which do actually come in contact in a given time with the condensing surfaces are as the arithmeridones, and the number of their returns as the lengths of the paths inversely, the same as in the first strata. Therefore, if we carry on the reasoning to the *n*th strata, from which in the given time no condensation takes place, the sums of the condensations from all the strata, or the increments of the condensations in the two media will have the same ratio as the condensations from the first strata; that is, the ratio of the elasticities.

Prop. X. Theor. VIII.

If the megethmerins of two portions of the same vapour confined over equal and like condensing spaces be equal, I say the contemporaneous increments of condensation will have a ratio equal to the triplicate ratio of the temperatures.

The forces to produce union at the times of collision being alike, the ratio of the contemporaneous condensations would be equal to that of the numbers of times the most adaptable sides of two corresponding particles in the two media come in contact with similar parts of the condensing surfaces. But the megethmerins being the same, the times of contact are in a ratio equal to that of the velocities or temperatures; and the number of times a particle turns a particular face towards a given part of space in a given time, is as its velocity about its centre of gravity. But this velocity is as the force which occasions it, namely, the intensity of collision or temperature of the medium. Therefore, the forces to produce union and the megethmerins being alike, the ratio of the contemporaneous condensations will be equal to that of the squares of the temperatures.

Again, the particles being nearly equal, and the unions being supposed to take place when they are moving nearly parallel and towards the same parts, the tendency to union at each collision, and, therefore, the number of unions in a given time, will be as the force producing that tendency; that is, by what I have shown in my former paper, as the temperature of the medium. Compounding this ratio with the other before found, the ratio of the contemporaneous condensations will be equal to that of the cubes of the temperatures. Q. E. D.

PROP. XI. THEOR. IX.

If portions of the same vapour be confined in vacuo at unequal temperatures over equal spaces of like condensing surfaces respectively at the temperatures of the vapours, the ratio of the contemporaneous increments of condensation will be equal to that compounded of the ratio of the temperatures and the ratio of the elasticities of the vapours.

Let C, C′, denote the condensations, T, T′, the temperatures, and E, E′, the elasticities of the vapours; and let *c*, *e*, denote the like things of another vapour, supposed to have the temperature T of the former, and the megethmerin of the latter.

Then by Prop. IX.

$$C : c :: E : e$$

And by Prop. X.

$$c : C' :: T^3 : T'^3$$

But by Cor. 6, Prop. IX. of my former paper,

$$T^2 : T'^2 :: e : E'.$$

Compounding these ratios we get

$$C : C' :: E\,T : E'\,T\,. \quad \text{Q. E. D.}$$

Cor.—Because E is as $M\,T^2$ by Prop. VIII. of my last paper in which M signifies the megethmerin; and because in the same vapour the megethmerin is as the specific gravity S, we have C as $E\,T$ as $S\,T^3$. That is, the incremental condensation is as the specific gravity and cube of the temperature of the vapour conjointly.

On this Cor. and the principle of vaporous tension, which I shall presently demonstrate, depends the whole theory of hygrometry, when properly considered, a subject which now I must dismiss with the bare mention of its name.

PROP. XII. THEOR. X.

If any portion of vapour be mixed with any quantity of incondensible gas at the same temperature, and be contained with it in a given space over a given condensible surface, the incremental condensation will be the same as if there was no gas present, and the whole space was occupied with the vapour alone. Or the incremental condensation, *cæteris paribus*, of any mixture of vapour with gas will be as the elasticity of the same quantity of vapour occupying the same space as the mixture does.

Conceive the two airs to be so divided in strata parallel to the condensing surfaces, that if the particles of each air were uniformly disposed throughout the corresponding strata, each air would be the same number of particles thick. Then because the gas itself does not condense, the ratio of the incremental condensations from the first stratum in each air, will, by what we have before shown, be equal to the ratio compounded of the ratio of the numbers of vaporous particles in the stratum, and the ratio of the numbers of returns which two corresponding particles make in the same time.

Take A' for the megethmerin of the vapour in the vacuum, and A for that of the mixture of vapour and gas; and $\sqrt[3]{A'}$ will be as the number of strata of the mixture in a given length, and $\sqrt[3]{A}$ as the same thing of the vapour. And because the megethmerin of the vapour must be equal to the number of particles of vapour in the same space in the mixture, $\frac{A'}{\sqrt[3]{A}}$ will be as the number of vaporous particles in the first stratum of the mixture, and $\frac{A'}{\sqrt[3]{A'}}$, as the same thing in the pure vapour; therefore, the ratio of these numbers is equal to that of $\frac{A'}{\sqrt[3]{A}}$ to $\frac{A'}{\sqrt[3]{A'}}$, or of $\sqrt[3]{A'}$ to $\sqrt[3]{A}$. But the temperatures, and, therefore, the velocities of the vaporous particles being equal, the ratio of the numbers of returns to the condensing surfaces of corresponding particles will be equal to that of the paths described inversely, or to that of the cube roots of the megethmerins: or, in the present case, equal to that of $\sqrt[3]{A}$ to $\sqrt[3]{A'}$. Compounding this ratio with the last, it will make a ratio of equality. Therefore, the incremental condensation is the same in the first stratum of the mixture as in the first stratum of the vapour. By a similar train of reasoning, the same may be shown to be true with the second, third, and higher strata to the *n*th, from which no condensations may be supposed to take place. The sums, therefore, of all these corresponding condensations, that is, the incremental condensations of the two airs, must be equal. Q. E. D.

Cor. 1.—By this and the preceding proposition, it appears that if C be the incremental condensation of any vapour on a unity of surface in vacuo at the temperature T and elasticity E; and if S be the area of the condensing surface, C will be as **E S T**, which is a general equation from which all the phænomena of the condensation of vapours may be deduced; and is equally true whether the vapour be in vacuo, or mixed with any quantity whatever of gas.

Cor. 2.—From this theorem it follows, that the temperature remaining the same, the condensation of vapour mixed with gas is neither increased nor diminished by the elastic force of it in conjunction with the gas, but is entirely proportional to the elastic force it would have if it occupied the same space with all the gas withdrawn; a fact which phænomena confirm.

Prop. XIII. Theor. XI.

In every vapour confined over the surface of its generating fluid, the temperature being the same, the megethmerin is always the same, however much the space occupied by the vapour is augmented or diminished, provided there be a sufficient quantity of fluid to supply the vapour required. Or at the same temperature, the elasticity is the same whatever be the space the vapour is made to occupy.

I have already shown that the higher the temperature of any fluid, the greater is its incremental evaporation, and *vice versâ*. But at the same temperature, the cause being the same, the increments of evaporation must be the same. I have likewise shown that the incremental condensation of any vapour, the temperature being the same, is as its elasticity. If, therefore, a quantity of vapour be confined over its fluid, the excess or defect of the evaporation of the fluid will increase or diminish the quantity of vapour until its condensation be equal to the contemporaneous evaporation of the fluid, or until the elasticity of the fluid has attained a certain force. When the elasticity has attained this force, if it be attempted to be increased or diminished by forcing the fluid into the same space occupied by the air, or by withdrawing it, a part of the vapour will be condensed, or a part of the fluid evaporated, until the equilibrium of condensation and evaporation, and, therefore, the proper elasticity of the vapour, be restored.

This elasticity, which makes the condensation of the vapour equal to the evaporation of the fluid, I shall henceforward, after the example of Mr. Dalton and M. Biot, call the tension of the vapour.

Prop. XIV. Theor. XII.

The temperature being the same, the incremental evaporation of any fluid is the same, however great or however small the aëriform pressure on its surface, or however great or however little the quantity of superincumbent vapour.

This theorem is to be understood as true only in evaporation strictly so called, that is, a decomposition at the surface, and not that interior decomposition which produces ebullition.

Because the pressure of an incumbent atmosphere acts by repeated impulses on the surface of the body, and not by a constant pressure surrounding and squeezing together the parts of the particles, the action of such an atmosphere, provided its temperature be the same as that of the fluid, however great its compression, will have no influence to accelerate or retard the corpuscular decomposition, or the absolute evaporation of the fluid. And since this is true of superincumbent airs in general, it must be true of superincumbent vapours; and, therefore, the quantity of superincumbent vapour has no effect on the incremental evaporation of the fluid. Consequently the temperature being the same, every fluid evaporates equally fast in a vacuum, and under an atmosphere of any compression; and equally fast in what is called a damp and in a dry atmosphere.

Cor. 1.—Hence if the incremental evaporation of the fluid be denoted by F, and the contemporaneous condensation of the superincumbent air by A, the apparent evaporation of the fluid will be $F - A = F - E\,T$, Prop. XI. Therefore, though the temperature being the same, the fluid would continue to evaporate equally fast under every circumstance of humidity and pressure in the superincumbent air, the apparent evaporation would only be as the excess of the evaporation of the fluid above the condensation of the vapour in the air. Let τ be the tension of the vapour in vacuo at the temperature T of the fluid, then, by Prop. XI. the apparent evaporation of the water is as $(\tau - E)\,T$, E being the elasticity the vapour in the atmosphere would have, were it left to occupy the same space with all the air withdrawn.

From this last formula it appears, that if the vapour in the atmosphere be very small compared to the evaporation from the fluid, the evaporation will be nearly as the tension of the vapour corresponding to the true temperature of the fluid, and the true temperature of the fluid conjointly. Mr. Dalton, to whose ability we owe almost the whole of our knowledge of the laws of evaporation, has found that water at 212° Fahr. evaporates at the rate of 30 grains per minute. At this temperature the tension of vapour is 30 inches. Let t be the true temperature corresponding to 212° Fahr. then the evaporation of water in a dry atmosphere at any other temperature, in grains per minute, is equal to $\frac{\tau\,T}{t}$ grains, supposing the atmosphere in which Mr. Dalton operated, was perfectly dry.

The following table, whose first, third, and fourth columns I have copied from M. Biot's Traité de Physique, will show how this theorem agrees with the actual experiments of Mr. Dalton.

Temperature of observations in degrees of Fahrenheit.	True temperature.	Elastic force of the vapour in inches of mercury.	Evaporation		Errors of theory compared with experiment.
			Observed in grains.	Calculated from theory.	
212°	1172·6	30·0	30·0	30·00	·00
180	1143·8	15·15	15·0	14·78	—·22
164	1129·2	10·41	10·0	10·02	+·02
152	1118·0	7·81	7·5*	7·45	—·05
144	1110·5	6·37	6·0	6·03	+·03
138	1104·9	5·44	5·0	5·13	+·13

Mean error, —·015

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* In this fourth experiment, I think the printer of M. Biot's or of Mr. Dalton's work must have made an error, which I have endeavoured to rectify. It stands 8·5 in the Traité de Physique; but from the well-known care and skill of Mr. Dalton, and the relation of the preceding and subsequent experiments with the tension in the third column, I would venture to say 8·5 cannot be correct. There is no question, I think, but 7·5 is what Mr. Dalton observed; and that the 8 crept in by the inattention of the compositor of one of the works.

We see here another beautiful accordance of our theory with facts, and on a subject that, perhaps, philosophers would hardly expect.

It has been supposed that the evaporation is strictly proportional to the tension, but the preceding table evidently shows, that the formula I have given involves the true theory. When experiments on evaporation are carried to much lower temperatures, or made in atmospheres of high temperatures, it is necessary to make an allowance for the proper tension of the vapour in the atmosphere. Mr. Dalton it seems made his experiments in temperatures too low to produce any sensible effect on the numbers of the preceding table; but in carrying the comparison to experiments of much inferior temperatures, the computations would generally come out higher than experiments.

If we want a theorem to express the evaporation, taking into account the condensation from the aqueous vapour in the atmosphere, and supposing of course that no part of the evaporated vapour recondenses on the water, or that, if it does, it is proportional to the quantity evaporated, our theory gives $\frac{\varepsilon}{t\,\tau'}(T\,\tau - t\,E)$ where ε signifies the absolute evaporation in any given measure or weight at some fixed temperature t, for a determinate small portion of time; τ' the tension of the vapour at the fixed temperature t; T the temperature of the water, τ the tension of its vapour at this temperature T; T′ the temperature of the atmosphere, and E the elasticity the vapour in the atmosphere would have, were it to occupy at the same temperature T′ the same space in a vacuum.

This theorem is easily reduced to that I have given above; and by a little transposition many interesting things may be shown to flow from it; but I hasten to other matters.

Cor. 2.—By the theory I have given, it appears that the increment of condensation depends exclusively on the temperature and space occupied by the aqueous vapour; and it is of no consequence whether in that space there be none, little, or much, of any other incondensible aëriform body. Hence follows what has been thought a very singular property of vapours; namely, that mixed with any gas in a sufficient proportion they are capable of supporting an indefinite weight, but alone only a certain one according to the temperature. The complete solution of this paradox is, that water at a given temperature evaporates at a given rate whatever be the superincumbent pressure: and vapour at a given temperature condenses at a rate inversely proportional to the space through which it is diffused, without any respect to the elasticity of the mixture of which it is a part. Therefore when this space is too great, the evaporation of the water, if there be any water, will gain on the condensation of the gas, until the one balances the other, and then the vapour will have attained its proper tension; but if the space be too little, the evaporation from the water will fall short of the condensation of the vapour, or the re-evaporation of the vapour, as it condenses on the sides of the vessel will be less than the condensation; and thus the apparent condensation will increase until the vapour is reduced to a proper tension.

By these views and the theorem given towards the end of the preceding corollary, it is easy to ascertain from the celerity of evaporation, the quantity of aqueous vapour at any time in the atmosphere.

This theory demonstrates the absurdity of the doctrine of atmospheric saturation by aqueous vapour. If we admit such a doctrine as this, we must likewise admit the saturation of a vacuum; for even a vacuum at a given temperature can only contain a definite "dose." But if we are determined to have saturation, to make the thing agree with phenomena, we must introduce the saturation of space. We must grant what I fear will not easily be acknowledged, that space can be saturated with one thing, and at the same time admit any quantity of another without inconvenience or confusion. Such an hypothesis as this carries, perhaps, too much the face of improbability; but if we do not discard saturation, I cannot see how we can reject it.

Cor. 3.—When the megethmerin of the vapour in the atmosphere is equal to that which is due to the tension of the vapour at that temperature, the condensation upon any non-absorbent will just balance the re-evaporation; and the sides of any vessel containing such an atmosphere would neither appear remarkably damp or remarkably dry, but any absorbent in such an atmosphere would be completely damp. If, however, the air was compressed but ever so trifling, the megethmerin of the vapour would become too great, some of the vapour would condense, and the surface of the body would appear to be covered with a slight dampness or dew. Were the compression carried farther, the quantity of dew would of course increase, and would be proportional to the degree of compression from the point where the condensation and evaporation balance. On the contrary, the aridity of the sides or air would also be increased in proportion to the rarefaction from the same point of balance or equilibrium. We can, therefore, extract vapour of any kind from a body by simply rarefying the air in which it is placed. And if this rarefaction at proper intervals be carried to a sufficient length, we may produce an effectual desiccation.

By the application of this principle, we arrive at a simple method of obtaining an almost perfect vacuum. If a body which evaporates with tolerable ease containing a sufficient quantity of humidity be introduced into a receiver by means of an apparatus, by which it may be easily enclosed or exposed, a thing which it is no difficult matter to contrive, the air being as much as possible exhausted, and the damp body exposed, the evaporated vapour will mix with the remaining atmosphere and increase its elasticity; so that after a little time the pump will again act, and a considerable portion of the remaining atmosphere may be extracted with the vapour. This process repeated at proper intervals, the atmosphere may be exhausted to almost the last particle. The damp body being now withdrawn, and a desiccative exposed, either by letting off mercury with which it may have been covered, or otherwise, the whole of the vapour might be absorbed, and nearly a Torricellian vacuum obtained. A process something like this has been used by Smeaton and some of the French philosophers, but being unacquainted with the true nature and laws of vapours, they could not bring their ideas to that practical perfection which it is hoped the laws here developed will enable them to do.

A phænomenon of the kind I have just been mentioning has been remarked by M. Biot, which often puzzles young philosophers. When the air has been extracted by the air-pump until it has no longer strength to raise the valves, we find, if the apparatus be let stay for some time, that the pump will act again, and extract a considerable quantity of air, though we are certain the apparatus was so tight that none could have introduced itself to the interior. This arises from the slow and gradual conversion into vapour of the humidity which almost always adheres to the interior and sides of the apparatus.

Since the megethmerin of any vapour may be so increased by compressing the air in which it is contained that the momentary condensation shall at length exceed the re-evaporation; and since we know that this will be the case when this megethmerin exceeds by ever so trifling a degree that which is due to the proper tension of the vapour at the temperature of the atmosphere, we can evidently, by compressing the atmosphere until a slight dew just begins to appear, and by determining the amount of that compression, ascertain the dampness or quantity of vapour in the atmosphere. Let m' be the megethmerin of the vapour at its proper tension, and m that of the vapour in the atmosphere; and let the dew just begin to appear when the atmosphere is compressed in the ratio of r to 1. Then the megethmerin m must by this compression be increased in the ratio of 1 to r, and becomes $r\,m$, and must be equal to m'. Whence $m = \frac{m'}{r}$. Therefore, m' and r being known, m is known, that is, the quantity of vapour in a given volume of the atmosphere.

Cor. 4.—There is another method of determining the quantity of aqueous vapour at any time in the atmosphere by pouring into a glass vessel water at different temperatures inferior to that of the atmosphere, and observing at what temperature of the water the vapour of the atmosphere just begins to be deposited in the form of dew on the sides of the glass. At this temperature, the megethmerin of the vapour in the atmosphere must evidently be equal to that due to the tension of vapour at the same temperature. Let us denote the higher temperature, or temperature of the atmosphere by t, the corresponding tension of the vapour by τ, the megethmerin of the vapour in the atmosphere by M, and of the vapour at its proper tension by m; and suppose E the elasticity, the vapour in the atmosphere would have occupying at the same temperature the same space in vacuo. Also let τ' be the proper tension of the vapour at the lower temperature t', or at that temperature at which the vapour of the atmosphere begins to be sensibly deposited, and m' its megethmerin. Then m' being the megethmerin of the vapour in the atmosphere at the temperature t', by the laws of gases, and supposing the atmospheric pressure the same,

$$m' : M :: t^2 : t'^2$$

But $$\tau' : E :: m' t'^2 : M t^2$$

by the laws of gases in my former paper; consequently $\tau' = E$; that is, the elasticity of the vapour in the atmosphere, had it the same temperature and megethmerin in vacuo, would be equal to the tension of the vapour at the temperature at which it sensibly begins to be deposited or to form dew. Therefore $\tau : E$ or m: M :: $\tau : \tau'$, and consequently the humidity of the atmosphere is equal to $\frac{\tau'}{\tau}$, perfect humidity being, at the temperature t, supposed to be when the megethmerin is the same as that of the vapour at its proper tension.

Mr. Dalton, and after him M. Biot, in the celebrated Traité de Physique, has given a different method of determining the humidity of the atmosphere; but these philosophers not having the advantage of a knowledge of the true nature and laws of aëriform bodies could scarcely avoid falling into some inconsistencies, where they had not experiment to direct them. Mr. Dalton, says M. Biot, "détermine le degré précis du thermomètre, où l'humidité de l'air commence à se déposer en rosée sur les parois extérieures du vase. Quand il connait cette température, il calcule la force élastique de la vapeur qui y correspond, et cette force, ramenée à la température extérieure par les lois ordinaires de la dilatation des gaz, est précisément celle de la vapeur aqueuse qui se trouve actuellement dans l'air." Now I do not know of any reason why this should be the case. On the contrary, experience proves that in a given space the quantity of vapour over its generating fluid is not augmented nor diminished by the presence or absence of any quantity of atmosphere; consequently, if at the temperature where sensible deposition begins to appear all the atmosphere could be taken from the vapour, and this left to occupy the same space, its elasticïty and megethmerin would be the same as the tension and megethmerin of vapour over its fluid in vacuo, which precisely accord with our theory. But the pressure of the atmosphere being the same on the compound mass of vapour and gas at the two temperatures, the elasticities of the compound must be the same at both temperatures. Therefore, and because the proportions of atmosphere and vapour are supposed to be invariably the same, the elasticity of each must be the same at either temperature; and, consequently, the elasticity of the vapour in the atmosphere is, as our theory determines it, the same as the tension of the vapour corresponding to the temperature at which sensible deposition commences, and not greater or less. It was for want of considering this equality of pressure in the atmosphere, I conceive, that the above two able philosophers, and all who have followed them, have run into errors on this part of the subject.

A pretty instance presents itself for verifying the truth and consistency of this and the preceding corollaries to those who choose to try it. In the preceding cor. I have said that sensible deposition will commence where the megethmerins of the vapour in air and in vacuo are equal; that is, when the elasticity proper to the vapour in the atmosphere is equal to the proper tension of the vapour at that temperature. But τ' is by this cor. the proper elasticity of the vapour in the atmosphere, which, to make it equal to τ, must be multiplied by $\frac{\tau}{\tau'}$; that is, to produce sensible deposition at the same temperature, the air must be condensed in the ratio of the tension corresponding to the temperature at which sensible deposition takes place to the tension corresponding to the temperature of the atmosphere.

We perceive by the present corollary that the apparent humidity of the atmosphere does not depend on the absolute quantity alone of vapour it contains, but on the quantity as compared to that which could exist over water in vacuo at the same temperature, or on the absolute quantity of vapour in the atmosphere and the temperature conjointly. Hence the apparent humidity is not a measure of the absolute quantity or proportion of aqueous vapour at different temperatures; and, consequently, all our hygrometrical instruments which are grounded on the principle of apparent humidity do not in any wise serve to discover the real quantity of humidity, unless a due regard be had to the temperature. Therefore, in all cases where the absolute humidity is sought, the temperature must be taken into account. This may be done for all temperatures under the boiling of water by the table computed by Mr. Dalton from his experiments; and for these and all other temperatures by a simple theorem I shall presently give.

By this theory, we perceive that a warm atmosphere with the same apparent aridity contains more vapour than a colder; and hence in any particular place the day air contains more humidity than the night, the evening than the morning, the summer than the winter air, and the lower regions than the higher. Now philosophers, I believe, tell us, that there is the same proportion of oxygen to nitrogen in a given portion of atmosphere, whether it be taken within doors or without, from the cabins of sickness, or the mansions of health, from the highest regions of the air or the surface of the earth, from the sea or the land, from the torrid or the frigid zone. If this be the case, it cannot, therefore, be to any excess or defect of oxygen that the salubrity or insalubrity of the atmosphere in some places is owing. Does not the healthiness or unhealthiness of the air depend on its absolute and not comparative quantity of humidity? Granting such to be the case, we should say that high and elevated regions are more healthy than low ones; dry absorbent soils, as limy and chalky, than clay and marshy; dry and cold climes, than warm and damp ones. And in the same place, we should say the morning air after a copious deposition of dew is more healthy than the evening, when the dew is beginning to precipitate; the winter more healthy than the summer; a cold dry atmosphere more healthy than a warm close one; a windy agitated air, which carries off, and thus prevents from being inhaled, the continual exhalations from the body, more conducive to health than a quiet still one. We should likewise say, that large confined towns in which the air is generally warmer, more quiet, and more humid, from the continual culinary operations, and the great quantity of animal exhalations, are much more insalubrious than open country situations; where there is not that immense collection of animal matter; where the cold is commonly greater; where no artificial heats are kept up; where humidity following the ordinary laws of nature can be deposited, and thus, for some hours at least, render the atmosphere more dry and more pure; where the winds can blow with unrestrained freedom; and where the air, if I may so express myself, has room to be purged and to be purified. Now the truth is, that all these conclusions are known to be correct. Dry soils and high situations have always been preferred for health. Damp low islands where fogs prevail are usually allowed to be pernicious. The morning air has been always considered one of the greatest promoters of health; which has received a beautiful illustration from some researches of one of our judges, who found that out of 800 individuals that had attained an extended age, their habits agreed in one point only, early rising. A brushing cold day is generally conceived more healthy than a warm muggy one; and the country is ever allowed more salubrious than the city. In those climes, as in Abyssinia, &c. where the rains last for a considerable period, the sickly seasons commence and terminate with the rains. Even a lime kiln, I have been informed, contributes to the healthiness of the neighbourhood. This probably depends chiefly on the desiccative property of the lime absorbing and condensing within itself the vapour of the atmosphere, though lime indeed has another property not less salubrious, when exposed to the air, that of absorbing carbonic acid gas; but the beneficial effects of this are, perhaps, more than counteracted by its absorption of oxygen. Many other facts might be adduced which experience has taught us, to show that insalubrity depends chiefly on the humidity of the air, and, therefore, most probably, on the absolute quantity of vapour it contains. I do not mean to say that the unhealthiness of air is exclusively due to its aqueous vapour. Probably other matter which it releases in the violence of its efforts to evaporate, and which consequently ascends with it, or most likely adheres to its particles, forms a more pernicious element than pure vapour alone. With this view we should still say that in any particular place the unhealthiness of the air depends on the absolute quantity of its vapour; but on comparing the airs of different places, we should conclude, that the insalubrity depends conjointly on the absolute quantity of vapour and the quantity and inimicability of the other matter it contains. Hence one reason why the vapour, and, consequently, the atmosphere, of the sea may commonly be more healthy than that of the land; for the vapour raised from the sea is most probably more faithful to the pure constituents of water, oxygen and hydrogen, and much less contaminated with extraneous matter than the vapour raised from the land. The breeze, however, which usually prevails on sea, by incessantly changing the air we breathe, and carrying off any noxious exhalations, constitutes no immaterial part of its healthiness. By these views we see also why one place shall be so uniformly healthy, and another so uniformly unhealthy; and why a place which is equally cold, equally high, and equally dry, with another, shall, notwithstanding, be less friendly to the human constitution; for other things being the same, the salubrity of the air depends on the soil which gives birth to its vapour. Supposing, however, a perfect equality in other matters, phænomena appear to me to mark aqueous vapour, however pure it may be, as by no means beneficial to health. If this be the case, it will remain for philosophers to determine in what this pernicious principle consists, and the mode of its operation. Judging from the nature of the elements of vapour, I should be disposed to attribute it to the hydrogen. But this is little more than mere conjecture. Oxygen itself, combined with hydrogen, in the form of vapour, may be, perhaps, as inimical to the human constitution as hydrogen or nitrogen alone. Speculations of this kind, however, belong not to the mathematical but the medical philosopher. I have gone a little out of my way for the sake of exciting among those, in whose province it more immediately comes, successful inquiry into a subject which is of the utmost importance to mankind. Whether my views are deserving attention, and my wishes shall be seconded by the efforts of others or not, can be to me, as an individual, but of little consequence. The motives which prompted me to deliver my sentiments on this subject flow from an idea, which has ever appeared to me to form the true estimate of human attainments and discoveries; namely, that they can be but of little value, however great, or however splendid, unless they conduce to the comfort, or add to the convenience and benefit of mankind.

From this corollary, a curious method presents itself of drying a room with cold water. For if a large vessel of water below the temperature at which the vapour of the atmosphere in the room would be sensibly deposited, be brought into the room, the water would rapidly condense all the vapour which contributes to make its elasticity in a vacuum more than the proper tension of vapour at the temperature of the water. If, therefore, the temperature of the water be sufficiently low, a very considerable part of the vapour may be condensed, and the room rendered apparently very dry. Such a method as this is familiar to philosophers, but they have not yet succeeded to bring the effects to mathematical rules. The thing, however, is very easy. Let τ be the tension of vapour corresponding to the temperature of the room, τ' the same thing corresponding to the temperature at which sensible deposition commences, and τ'' the tension of vapour at the temperature of the water. Then, by this cor. the humidity of the air is $\frac{\tau'}{\tau}$, and the humidity after the water has condensed all it can $\frac{\tau''}{\tau}$. The effect of the water, is, therefore, $\frac{\tau' - \tau''}{\tau}$. If τ'' be less than τ', the water contributes to dry the room; if $\tau'' = \tau'$, it produces no effect; but if τ'' exceed τ', it tends to make it damper.

Do not these views present us with a simple efficacious method of rendering the air of a room where there is a large company more healthy by condensing the noxious vapours as they ascend on a current of cold water, or on a surface of broken ice near the ceiling? By either of these methods, but especially the last, the air of a room, if the ice be placed in different parts before the company enter, may be preserved wholesome where the party is large without any sensible inconvenience or offence. The ice, if in large pieces, will take a very considerable time to liquefy, and the condensations from the exterior of the vessels may be caught and prevented from falling on the company in reservoirs of obvious contrivance beneath.

A method first, I believe, proposed by Dr. Franklin, has, within these few years, been practised of heating rooms with air that has passed through a warm tube. Such a method is very simple, and, perhaps, not very insalubrious, provided the pipe be not too hot, and the air come from a dry and healthy region; but if the pipe derive its air from a damp atmosphere, or a low humid situation, it is fraught with much inconvenience and mischief. I recollect hearing some of my friends in Bristol, who had rooms heated this way, with pipes receiving their air from near the river, complain much of an unpleasant dampness the heated air brought with it, while another gentlemen, of considerable science, whose pipe was in a drier and more elevated region, assured me he never perceived the least tendency to dampness, much less did he experience any inconvenience from such a thing. A simple desiccative substance put near the mouth of the tube in the former instance, and frequently renewed, would probably obviate the dampness complained of. However, under all circumstances, the constant renewal of air which fire occasions appears to me much more conducive to health, if it were only for dissipating and carrying off the incessant exhalations of the body, than this new method. To be convinced of the utility of fire in changing the air, one need only enter a room from the fresh air where there is a fire and company, and afterwards where there is an equal company and no fire, and he will not want to be told which is the more healthy apartment.

Cor. 5.—The ready intermixture of gases one with another, and of aqueous vapour with the atmosphere, has long been a problem of considerable difficulty with philosophers. To account for phænomena of this nature, they have created a kind of attractive affinity between the parts of different bodies, and yet with this assistance they find it no easy matter to give to their views even the semblance of probability. Our theory accounts for such phænomena from the nature of the bodies, and, therefore, explains the mixture and ascent of aqueous vapour in the atmosphere from the principles of constitution. In every evaporation, however, the evaporated particles, by striking in all directions against the particles of the atmosphere, must necessarily occasion reflections of many of the vaporous particles to the surface of the fluid. Of these reflected particles some of them must evidently be recondensed on the fluid; and as the chance of striking with their condensible sides is in proportion to the quantity reflected, and the quantity reflected to the quantity evaporated, the quantity recondensed will be in proportion to the quantity evaporated. Therefore these recondensations will not any ways affect the truth of the propositions I have advanced.

If evaporation takes place in a still atmosphere, the recondensation will proceed without interruption; but if a current of air passes over the surface of the evaporating body, the vapour as it is raised will be in part carried off, and the recondensation on a limited surface diminished. The greater the current the greater will be the effect, and the less the amount of recondensation. In moderate winds, the diminution of the recondensation will be accurately proportional to the current, if the evaporating surface has a sensible extent; but when the velocity of the current is very great, or its ratio to the linear extent of the evaporating surface exceeds a certain quantity, I find the decrement of recondensation follows a different law. Of these things, however, I may treat more fully hereafter, when I shall be better provided with experiments to verify the laws I shall develop.

Because in moderate currents, that is, in all such as we usually experience, the decrement of recondensation is proportional to the whole recondensation and the velocity of the wind conjointly, and the recondensation is proportional to the whole evaporation, the apparent increment of evaporation occasioned by a current of air over the surface of an evaporating fluid, is proportional to the velocity of the current and the evaporation conjointly. This conclusion agrees with the views of Mr. Leslie obtained from experiment.

I have yet been speaking of currents parallel to the surface of the fluid, but mathematically looking at the subject, there seems to be another source of anomaly in evaporation. When the air blows parallel to the surface, the mean collisions of the particles of air against those of the fluid are, perhaps, not increased by the velocity of the current; but if the air blows obliquely on the surface, it seems reasonable to suppose that the intensity of collision is augmented by a quantity proportional to the cosine of the angle of incidence and the velocity of the current conjunctly. This increase of collision is equivalent to an increase of temperature, and, therefore, ought to produce an increase of evaporation. Assuming what, perhaps, does not in this case materially differ from the truth, that the increment of evaporation depending on this cause is proportional to the increment of collision, the increment of evaporation will be equal to the continued product of some constant b, the cosine of the angle of incidence, and the velocity of the wind. But under these circumstances of oblique currents, the incremental evaporation before alluded to is also equal to the continued product of some constant a, the sine of the angle of incidence, and the velocity of the wind. Therefore, the maximum of apparent evaporation will be when the sine of the angle of incidence is to the cosine as a is to b.

Philosophers who may be engaged in experiments on evaporation will not, perhaps, take amiss my recommending to their attention the influence of oblique currents on the apparent acceleration of evaporation. Experiments of this kind, and a multitude of others I have in contemplation to make, will, I fear, by me necessarily be subject to lie long on the list of intended consideration. Philosophical experiments, where delicacy and precision are required and united to an almost boundless range of view, and where the ardour of research is damped by unusual discouragement, need more than the efforts of a solitary individual to compass and to carry into successful execution. Though since my arrival at Cranford no person could have been more fortunate in a part at least of his scientific connexion, yet I seriously feel the want of those good offices and assistance in my philosophical inquiries the friends I have left in Bristol were ever ready to grant me; and none do I feel the want of more than those of my respected friend, John Hare, Jun. Esq. a gentleman who, though taking a leading part in the most extensive concern of its kind in England, can yet find time to attend to the advancement of science, and to promote with unlimited zeal every thing which has the prospect of benefit or utility to science and the community.

We see by this corollary why a current of air in apparently increasing the evaporation likewise diminishes the temperature, and, consequently, the reason of the common method of cooling bodies, as meat, wine, &c. by wrapping them in wet cloths, and placing them in a current of air. Were the superincumbent air fraught with sufficient vapour to make the condensation equal to the evaporation, the momentary diminution of temperature resulting from the evaporation would be balanced by the contemporaneous increase of temperature arising from the equal condensation, and the fluid would remain of the same temperature. But were the condensation greater than the evaporation, the temperature of the fluid, as long as the excess continued, would rise; and if, on the contrary, the condensation was less, the temperature would sink. Now a current of air, we have shown, increases the apparent evaporation; it, therefore, diminishes the temperature.

Dr. Wistar, in the Transactions of the American Philosophical Society, informs us, that apparent evaporation takes place when the moist body is warmer than the medium in which it is enclosed; and, *vice versâ*, apparent condensation when the enclosed air is warmer than the body. This, under the circumstance of enclosing the air and a sufficient quantity of vapour being already raised, agrees with our theory; but if the air be unconfined, it may not hold good. Apparent evaporation in this case will depend on the quantity of vapour in the air immediately over the fluid.

A current of air is not from this theory indispensably necessary to increase the apparent evaporation. Simple agitation of the air, if it equally disperse the vapour and diminish the recondensation, will do as well, which agrees with experiments.

Because the apparent evaporation is increased by a current of air, it may happen that water of a lower temperature in a current or an agitated air may lose more by evaporation in a given time than water of a much superior temperature in a still atmosphere. It may likewise even happen, that ice itself will lose more weight by evaporation when placed in a strong current than water of a considerably higher temperature in a quiet air, particularly also if there be much vapour in the air. This indeed has been found to be the case. Calculations on this subject may easily be made, from the theorems I have given to show the force and extent of this view by any one who likes to amuse himself with them.

Considering that loss of temperature must always accompany evaporation when it is not balanced by recondensation, and knowing that the vapour in the air is scarcely ever sufficient to make the recondensation equal to the evaporation, it appeared to me that water ought generally to be of a lower temperature than the atmosphere, or than any other fluid which does not evaporate so fast. To satisfy myself, as I did not know of any experiments on the subject, whether this was the case, and how much it may ordinarily be, I made the following experiments.*

I took a glass bottle containing between 4 and 5 lbs. avoirdupois of mercury, and put it in a cup of spring water, in order to produce an equal temperature in both. After it had remained here for some time, half an hour or more, I took four wine glasses, two of one kind and two of another, two thin, and something of a semi-prolate-spheroidal figure, and two thicker approaching the figure of common beer casks, with about a third of the upper part cut off. These glasses, which I knew were as nearly as it was possible to have them of the same temperature, because they had been together for days in the same place, I put in a row with the two thin glasses on the right hand on a table in the centre of a room without fire, about 13 feet square. The right hand glass, and the next but one, were nearly filled with mercury, and the other two were similarly filled with the water I have mentioned. By this arrangement I had the glasses alternately filled with water and mercury, and one glass of each kind containing the same fluid; so that I conceived I should thus obviate any discrepancies arising from the sizes, figures, and thicknesses of the glasses, and their position in the room. Between the two middle glasses, on a book, I laid horizontally with its bulb projecting several inches off a delicate thermometer, on the scale of which I could easily read off to the one-sixth, or with care even to the twelfth of a degree of Fahrenheit. After allowing all things to remain in this position some time, I observed at nine hours, p. m.

The temperature of the room $68\frac{4}{6}$° Fahr. that of the

Mercury, left hand glass, $68\frac{3}{6}$; right hand glass, $68\frac{3}{6}$
Water .... $67\frac{3}{6}$; .... $67\frac{4}{6}$

This day had been exceedingly warm and dry. Letting all things remain in their present position until a quarter before six, the next morning, I then found

The temperature of the room, 66°.

Mercury left glass, $66\frac{4}{12}$; right glass, $66\frac{4}{12}$
Water .... $65\frac{8}{12}$; .... $65\frac{8}{12}$

At $8\frac{1}{2}$ hours, a. m. temp. of the room was $65\frac{1}{3}$°.

Mercury, left glass, 65; right glass, 65
Water............ $64\frac{1}{3}$; .... $64\frac{1}{3}$

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* Since writing this, I find an experiment of this description has been made by Dr. Wells on a more extensive scale: his object, however, was somewhat different to mine.

At $10\frac{1}{2}$ hours, a.m. temp. room $67\frac{2}{6}°$.
Mercury, left glass, 67; right glass, 67
Water .... $66\frac{1}{6}$; .... $66\frac{1}{6}$.

At 1 hour, p.m. temp. room, $69\frac{1}{6}$.
Mercury, left glass, $68\frac{5}{6}$; right glass, $68\frac{2}{6}$
Water .... 68; .... $67\frac{4}{6}$

At $8\frac{1}{2}$ hours, p.m. room, $65\frac{5}{12}°$.
Mercury, right glass, $66\frac{5}{12}$; left glass, $66\frac{2}{12}$
Water .... $66\frac{2}{12}$; 66

The next morning at six o'clock, the room was at $65\frac{8}{12}°$.
Mercury, left glass, 66; right glass, 66
Water. .... $65\frac{6}{12}$; . ... $65\frac{5}{12}$

At 9 a.m. the room was at $62\frac{4}{6}°$.
Mercury, left glass, $62\frac{3}{6}$; right glass, $62\frac{4}{6}$
Water............ $61\frac{5}{6}$; . ... $61\frac{3}{6}$

Between the two last observations, the window had been put open to try whether the damp air, owing to a little rain which was falling, would have any effect. A current of air had also sometimes been allowed to pass through the room by setting the door open, but I could not perceive it produced any considerable effect on either glass in particular. Generally, as the experiments indicate, the temperature of the right hand glass of water was less than that of the other, though the left hand glass was towards the window. This I attribute in part to the excess of superficial exposure which the water had in the left hand glass; the exposed superficies being nearly in the ratio of 3 to 2. The depth, however, of the water in the left hand glass was only about $\frac{12}{13 \cdot 5}$ of that in the other. In the glasses with mercury, the differences of the temperatures were, except in one or two instances, so trifling, that I could not appreciate them. Nor could I discover any inequality of temperature in the top, bottom, sides, or middle, of the same glass, though I took great care to detect it. It should likewise be mentioned, that in making my observations, I noted the temperature of the atmosphere before I removed the thermometer from the book; and to avoid being obliged to wipe or touch the ball of the thermometer, I immediately afterwards took the temperature of the glasses of mercury, deferring to take that of the water until the last, that no error might arise from the influence of aqueous particles adhering to the bulb of the thermometer. In the subsequent table, I have brought the results together, and computed the arithmetical means.

	Temp. of the room.	Mercury in the		Water in the	
		Left glass.	Right glass.	Left glass.	Right glass.
	$68\frac{4}{6}^\circ$	$68\frac{2}{6}^\circ$	$68\frac{3}{6}^\circ$	$67\frac{3}{6}^\circ$	$67\frac{4}{6}^\circ$
	66	$66\frac{4}{12}$	$66\frac{4}{12}$	$65\frac{8}{12}$	$65\frac{8}{12}$
	$65\frac{1}{3}$	65	65	$64\frac{1}{3}$	$64\frac{1}{3}$
	$67\frac{2}{6}$	67	67	$66\frac{1}{6}$	$66\frac{1}{6}$
	$69\frac{1}{6}$	$68\frac{5}{6}$	$68\frac{5}{6}$	68	$67\frac{5}{6}$
	$65\frac{5}{12}$	$66\frac{5}{12}$	$66\frac{2}{12}$	$66\frac{2}{12}$	66
	$65\frac{8}{12}$	66	66	$65\frac{6}{12}$	$65\frac{5}{12}$
	$62\frac{4}{6}$	$62\frac{3}{6}$	$62\frac{4}{6}$	$61\frac{5}{6}$	$61\frac{3}{6}$
Totals	$530\frac{3}{12}$	$530\frac{5}{12}$	$530\frac{6}{12}$	$525\frac{2}{12}$	$524\frac{7}{12}$
Means	$66\frac{27}{96}$	$66\frac{29}{96}$	$66\frac{30}{96}$	$65\frac{62}{96}$	$65\frac{55}{96}$

From this table it will be seen, that the temperature of the mercury differed at a medium so little from that of the atmosphere as not to exceed $\frac{3}{100}$ of a degree. The mercury, it would seem, was about this quantity higher than the atmosphere; but the difficulty of making the observation to this nicety was so great, the fluctuations of the temperature so considerable, and the difference itself so minute, that it could not be attributed to any other cause than the inappreciable errors of the observations, unless it be true, as Dr. Wells has sagaciously suggested, that a naked thermometer does not accurately indicate the temperature of the air in which it is placed, but a temperature commonly somewhat lower. Between the temperatures of the water and atmosphere, the difference was much more marked; at a medium it amounted to near three quarters of a degree. Such a difference as this is much too great to be laid to the errors of the observations, and can only be explained on the principle of evaporation, as I had anticipated from theory. I have not tried it in other fluids, but there seems no doubt but the same cause which produces an inferiority of temperature to that of the atmosphere in one fluid operates to the same effect in all those which evaporate at low temperatures. In general I expect the inferiority will be greater the greater the evaporation, and, therefore, I should think it would be more marked in ether than in alcohol, and more in alcohol than in water, under the same circumstances. Whether the effect would be materially different in winter it is impossible, from these experiments, to say. We should certainly expect it would be much less in cold damp weather than in warm and dry; but when I once set the window open, for two or three hours during the rain, I could not perceive any sensible difference. In fact, in the last set of observations, in which one would have expected the damp to have had the greatest influence, and consequently the inferiority of the water to be the least, the contrary was observed, and the inferiority was as great as in almost any other instance. Part of this, however, might, I conceive, have arisen from the strong current of air which, for a great while, was allowed to pass through the room; but further experiments are wanting to settle this matter.

It is observable that the inferiority of temperature is sensibly greater in the right hand water glass than in the left; but as the figures of the glasses were dissimilar, and I did not take the precaution to weigh the quantities of water, nothing can be determined from it.

I might now carry my inquiries into the phænomena of radiation and dew; and I might easily demonstrate the cause and consequences of things of this nature, and show that all the phænomena which have been observed by Leslie, Delaroche, Rumford, Dulong and Petit, Dufay, Williams, Barker, Wells,* &c. flow from the simple principles I have developed; but the full discussion of these subjects requires a great scope both of time and space. Should I not shortly enter on these things, I shall endeavour to unfold my views on these and a variety of others in a work I have in part promised my friends to bring to press as soon as 300 copies are subscribed for; namely, the Lectures I have for some years been accustomed to give my pupils on the various branches of Natural Philosophy. If this work be printed, I shall endeavour, while giving an easy and a popular elucidation of the various subjects in question, to make it not unworthy of the consultation of the philosopher.

(*To be continued.*)

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* When I had nearly closed the present part of this paper, I had the pleasure of receiving from John Wyatt Dobbs, Esq. through the medium of the Rev. H. S. Trimmer, a copy of Dr. Wells's excellent "Essay on Dew." The copious detail of phænomena in this work induced me to lay aside my thoughts of publishing a mere abstract of my ideas on the phænomena of dew in the present paper, and to reserve them until I should have a better opportunity of doing them justice. The same gentleman has also since had the goodness to send me a copy of the Philosophical Transactions, containing Dr. Ure's paper on the elastic force of vapours, which in the subsequent Proposition, I mention as not having at hand. Though the kindness of Mr. Dobbs has unfortunately come too late to afford me time for re-investigating the invariables of the theorem I have given in Prop. XV. it has, however, I am happy to say, prevented me from running into a great mistake with respect to the merit and accuracy of Dr. Ure's theorem into which I had fallen from a mistranscribed account of it by another.

ARTICLE VII.

On the Nature of the Pigment in the Hieroglyphics on the Sarcophagus, from the Tomb of Psammis. By J. G. Children, Esq. FRS. &c. &c.

(To the Editor of the *Annals of Philosophy.*)

DEAR SIR,

THE celebrated sarcophagus, discovered by M. Belzoni, in the, so called, Tomb of Psammis, is covered within and without with hieroglyphics, cut into the stone about 1-16th of an inch deep. The material of which this interesting monument of ancient sepulchral splendour is formed, is an immense block of white, translucent, carbonate of lime, of the variety, according to Dr. Clarke, called arragonite. The hieroglyphic figures are filled up with a pigment,* which, at present, is superficially almost black, but, when reduced to powder, has a dirty olive-green colour. Several of the figures are wholly without the pigment; and from many of the rest it has been partly detached, in some of which, the remaining portion exhibits a light-blue colour, mixed with the olive. In the following experiments, I was confined to the use of so very small a quantity, that I cannot undertake to give the relative proportions of the several ingredients of the pigment; but I hope they will, nevertheless, be sufficient to prove its composition and original aspect.

The pigment fused before the blowpipe, without addition, into an almost black slag.

It was easily pulverised, and when heated in a glass tube, a dark-coloured oily fluid distilled over, and a large quantity of dense, brown vapours, at intervals inflammable, was evolved. By this process, it lost about 10 per cent. of its weight, and left a black residuum in the tube.

The residuum was but little acted on by digestion in moderately diluted muriatic or nitric acid (or in a mixture of the two), which occasioned only a slight effervescence, and took up about 17 per cent. giving a bluish-green solution, which, on examination, contained copper and lime. (A part, at least, of the latter, may have been derived from the sarcophagus.)

The portion insoluble in the acids, and which constituted nearly three-fourths of the whole quantity, exhibited, when dry, a beautiful blue colour, not much inferior to the finest ultramarine, but somewhat deeper, and indestructible by a red heat. By fusion with nitrate of baryta, and the other processes usually adopted, I found it to be composed of oxide of copper, mixed with a large proportion of silica, and a small quantity of lime and potassa. The colouring matter is, therefore, evidently copper; and from the presence of the alkali, the pigment is probably an artificial production; and since I could discover no trace of any other metal, except an almost imperceptible one of iron, its preparation must, in all likelihood, have been conducted with equal skill and care.

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* **Dr. Clarke has mentioned this pigment, and stated its nature, in a note to his interesting memoir on arragonite, at p. 57 of the present volume of the *Annals of Philosophy*, which I was not aware of till after this paper was sent to the Editor. I am happy to find our results agree.**

The small portion soluble in acids seems to have derived that property from a partial decomposition of the pigment, which may well have been effected, by the action of the oil used as the vehicle to mix it up with, and other natural causes, in the long succession of ages that have rolled away since its first application.

Yours, &c.

J. G. CHILDREN.

ARTICLE VIII.

Further Remarks upon Mr. Herapath's Theory.

(To the Editor of the *Annals of Philosophy.*)

SIR,

THE following remarks were drawn up in continuation of some, which you did me the favour to insert in your number for September, before your number for October reached me. In that number I found Mr. Herapath's reply to my remarks, on which account I wish to premise, with respect to that reply, that, in the first place, I am not aware of any thing "ungentlemanly" in my remarks; and should any expression be thought so, I here beg to apologize to Mr. H. for it. I also wish to assure him that I am actuated by no motives of jealousy, and that any difference between him and the Royal Society can have had no influence whatever on my views of his theory, as I am not a member of the Royal Society, nor in any way interested in opposing his theory. My design was merely to state, without offence, one or two difficulties which occurred to me on reading his papers; and this I assure him I have done repeatedly, and with the greatest attention. The advice also which he gives me in his reply I have faithfully followed; and upon the most careful re-examination of both sides of the question, I must say, that with respect to the main and fundamental parts of his theory, I have found no reason to change my opinion. He accuses me of misrepresentation; I candidly and openly acknowledge that I have found one or two observations (but of little or no importance) which I would wish to retract. Mr. H. will easily perceive which I allude to; and I shall not trouble your readers with further particulars.

I will now proceed to a few further remarks, which I have been led to make partly from reading the continuation of Mr. H.'s communications, since the date of my former paper (which was sent in hopes of having it inserted in the *Annals* for August), and partly because some of my friends have thought that my observations on Mr. H.'s use of the term temperature needed a little further explanation. The substance of what I wish to observe respecting his law of temperature will, I hope, be distinctly understood if I state it thus:

He finds that, within a certain range, gases go on expanding nearly as the squares of a certain set of numbers.

According to his theory he considers it proved that gases expand as the squares of their temperature; that is, defining the temperatures to be the momentum of their particles. He, therefore, concludes, that that set of numbers represents the temperatures thus defined.

Now within the same range, the expansions are also nearly as the simple ratio of another set of numbers: that set of numbers are the common Fahrenheit temperatures; therefore, within this range, little evidence is gained either for or against his theory; and this range includes the temperatures at which almost all experiments are tried.

Again: he finds that the set of numbers first alluded to are such as are obtained in a given way from the Fahrenheit temperatures: hence he finds a point in Fahrenheit's scale which corresponds to a temperature of 0, or a point at which there will be no motion in the particles of a gas, and at which the volume of the gas will be 0 also.

If experiments agree with this progression of temperatures, what they prove is this, that the zero is fixed at such a point of Fahrenheit as to make the law of temperature agree with experiments within a certain range; that is, that the one hypothesis is so framed as to be consistent with the other. They prove that the theory of temperature agrees with observation, provided the amount of observation be measured according to a standard previously fixed, and fixed upon the assumption of that theory; for he is led to fix the zero where he does, by taking the ratio of the volumes of gas at the temperatures of freezing and boiling water; and assuming that the law of expansion and temperature holds good down to that point where the progression would end.

Unless it can be proved that such a state as absolute cold really exists, it cannot be admitted that these numbers represent the real temperatures, or absolute quantities of heat in the gases, or absolute intensities of motion reckoned from a point of absolute rest in their particles. They will only measure the temperature above a certain point, and not above real zero. None of the experiments which Mr. H. has mentioned prove the existence of such a point; and it appears to me, that, by his own theory, it may be proved that no such point can exist; for when the volume is 0, the gas does not exist. If it then begin to exist, it must begin by the production of a first atom infinitely small; but this atom must be supposed to have some heat, because the law of temperature is now beginning; and to constitute heat, according to Mr. H. there must be an intestine motion; and if so, this atom must consist of smaller particles, which is contrary to supposition. Thus the existence of such a point is disproved on the assumption of Mr. H.'s theory; which is, therefore, at variance with itself.

It may also be shown, in the same way, that by diminution of heat, the volume of the gas can never become 0, or, what is the same thing, can never cease to exist; for so long as there is any intestine motion left in it, there must be more than one infinitely small particle; and the total abstraction of all heat, or cessation of all intestine motion, cannot cause the disappearance of these particles.

These difficulties, however, are not peculiar to Mr. H.'s theory; precisely the same absurdity results from the common theory of repulsion. According to this theory, the smallest volume to which we can suppose a body reduced must still have some heat in it to give it a physical existence; and if we suppose no heat left in a body, there can be no separation of particles; they must be infinitely condensed, or, in other words, the substance cannot exist.

The fact appears to me to be, that whenever a real zero is said to exist, it is always on the assumption, avowed or implied, of some hypothesis concerning the nature of heat; and at the same time I am inclined to think, that on most such hypotheses, the doctrine may be as easily refuted as on this. If in such speculations we were to set out by defining the term heat in the only way in which it can be rightly defined, viz. from some of its observed properties or effects, we should soon find them all built without a foundation.

It would hardly seem necessary to remark, that in the present state of our knowledge the physical cause of heat is absolutely beyond the reach of investigation. If it were not that speculations concerning what is termed the point of absolute cold had received the sanction of some of the highest names in the scientific world, I would only require of any philosopher engaged in researches of this kind to define, with rigid precision, the term "real zero," or any equivalent expression which he may use; and I think it would be evident, that he could not define it without a reference to some hypothetical physical principle as the cause of heat. I remember long ago when I first studied Dr. Thomson's excellent "System," on coming to the chapter entitled, "On the Absolute Quantity of Heat in Bodies," being not a little astonished, at finding an author, in general so remarkable for his accurate adherence to fact, and rejection of theory, taking up a subject, which at first sight appeared to me utterly incapable of definition, and, therefore, improper for investigation; and still more surprised was I to find the names of such distinguished philosophers as those whose researches are there described, engaging in a speculation, which, it appeared to me, a simple attempt at definition would at once set at rest. Dr. T.'s remarks at the end of that chapter put a satisfactory termination to such inquiries; and I felt much satisfaction on looking into the last edition of his incomparable work to find the chapter wholly omitted.

We cannot take Mr. H.'s law of temperature as the true law, unless we are sure that it holds good at all points in the scale; but of this we cannot be sure, any further than within those limits at which experiments have been tried. How do we know that beyond those limits the law of expansion may not be modified, or some totally different law prevail?

I will now proceed to one observation respecting Mr. H.'s opinion of the doctrine of capacity. He states (see *Annals* for Sept. p. 204), that if the present doctrine of capacity were true, the results of mixtures, when the higher temperature belonged respectively to each of two bodies, should be equidistant from the arithmetical mean. I beg leave to suggest, that, according to the common doctrine, it by no means follows that if temperature be measured by the equable expansion of mercury, then the temperature of a mixture should be an arithmetical mean between the previous temperatures of the two bodies; for we do not know that the temperature of the bodies employed will be measured by their expansion. Heat does not in all cases cause expansion, and, therefore, we are not certain that part of the heat communicated by one body to the other may not have other effects without causing expansion.

Also if a certain degree of heat in one volume of a body be effective in expanding mercury to a given degree; and a certain degree of heat in another equal volume of the same body be effective in expanding mercury to another different degree; we may assume hypothetically, that when the two volumes are mixed, the mean of the two previous temperatures will, in some way, be effective in the mixture, but we can by no means be sure that it will be in such a way as to be displayed by a proportional expansion of mercury. Philosophers would be very properly employed in observing what the resulting temperatures are in such mixtures, and, consequently, what part of the heat is, after mixture, effective in expanding mercury; but to talk of the real quantity of heat in bodies is talking in terms which cannot be defined, and, therefore, the investigation is to no purpose.

I must also remark, that if Mr. H.'s theory be considered as established, there is no opposing theory exploded, as far as the experimental doctrines of capacity and latent heat are concerned;

for in those doctrines, no theory of the nature of heat is assumed; no supposition required but the mere facts, that equal degrees of heat communicated are not equally effective in certain different cases, in producing equal sensible heats. I allow that in the common views of these doctrines, the facts are, in some instances, perhaps injudiciously, but, at the same time, perhaps unavoidably, classed together, and described by terms which may sometimes mislead by their hypothetical appearance.

I cannot conclude without remarking, that if I rightly understand Newton's invaluable rules of philosophizing, on the nature and object of physical researches, it appears to me that in order to constitute a legitimate physical theory, two things are requisite: 1. That the causes assigned should be such as really exist; and 2. That they be sufficient to explain all the phænomena. With respect to the first of these qualifications, I contend that Mr. H.'s theory is defective; for his fundamental principle, the intestine motion of particles, as far as I know, has not been, and, I believe, cannot be proved to exist; still less is it proved that it uniformly accompanies expansion. With respect to the second requisite, my opinion must be obvious; and I would recapitulate it by saying, that, I think, he has not strictly deduced his conclusions from his first principles (see *Annals* for Sept. p. 224, at the top); and that his conclusions in general agree with experiments; but still that this is only within limits where the old views of the subject agree with them too.

I will take my leave of Mr. H. by first calling his attention to an excellent remark of Dugald Stewart, that what chiefly misled the ancients in their physical speculations was their inattention to what were the proper objects of inquiry, and their confused and wavering notions of that particular class of truths, which it was their business to investigate. I am far from wishing to apply this remark, in its full extent, to the present subject; but, I think, in general, it might be well to recommend it to the consideration of many philosophers of the present day. It would tend to confine philosophy to those subjects which it is its proper province to examine, and prevent much fruitless labour in endeavours to penetrate where the utmost strength of our faculties is unable to arrive at certainty. I am, Sir,

Your obedient servant, X.

Article IX.

SCIENTIFIC INTELLIGENCE, AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Education of Mechanics.*

A school for the education of mechanics has been established at Edinburgh, and upwards of 200 students have already enrolled themselves for improvement in their several trades.

The establishment of free drawing schools has long been a desideratum, and is no less essential in the more humble branches of mechanic industry than in the higher walks of our manufactures. Somewhat similar to this was attempted in France during the Buonapartean *regime*, and its success was very fully evinced by the great improvement displayed in their bronze and ormolu ornaments, as well as in their china manufacture, which for a time superseded those of our own country. From this it will be seen, that although we are not anxious to increase the already too numerous class of artists, who seldom pass beyond the bounds of mediocrity, we are yet persuaded that the tailor, or carpenter, would derive material benefit from employing his leisure hours in a school dedicated to those parts of design which are essential towards sketching with correctness the anatomy of the human figure, or the fitting up of a building.

II. *Application of the Air-Pump.*

Till within the last 10 years, the use of the air-pump had been exclusively confined to the service of the pneumatic chemist and philosophical experimentalist. Now, however, this valuable instrument is very generally employed in many of our manufactures. We believe that the sugar refiners working under Messrs. Howard and Hodgson's patent were the first who employed it in a large way. It is a fact very generally known that fluids boil at a lower temperature beneath the exhausted receiver of an air-pump than when exposed to the ordinary pressure of the atmosphere, and the sugar refiner, taking advantage of this principle, very readily prevents the charring incident to the old process. To accomplish this, it is merely necessary to enclose the pan containing the saccharine fluid in a close vessel, and by the continued action of an air-pump, the air is so far rarified as to produce ebullition at a temperature seldom exceeding 100° of Fahrenheit's thermometer.

This simple instrument has also been employed in the sizing and wetting of paper. In the former case, the paper is piled up evenly in a vessel capable of being rendered air-tight, and a vacuum being first formed, the size is introduced, which is afterwards pressed in by the force of the atmosphere; passing through the pores of the paper without injury to its fabric. In the process of dyeing, also, the air-pump has been found highly efficacious. In the ordinary way, the cloth is merely immersed in the dye, so that the internal part is of a lighter hue; but, in this case, the colouring matter passes through the entire fabric.

III. *Meridians of Greenwich and Paris.*

A vertical reflector, constructed by M. Mathieu, has lately been placed on Fairlight Down, Hastings. Its erection was superintended by the above gentleman and Capt. Mudge, of the Royal Engineers, who have chosen the same spot which was selected by Gen. Roy 30 years ago, to enable observations to be taken from the coast of France, for the purpose of remeasuring the distance between the meridian of the observatories of Greenwich and Paris. The light from the reflector is visible at the distance of 90 miles. It consists of four circular wicks, the largest of which is 10 inches in circumference. It consumes two quarts of oil in an hour, and is lighted an hour before sun-rise and sunset, and is kept burning for two hours. Capt. Mudge and M. Mathieu have since proceeded to join Major Colby and Capt. Kater in France.

IV. *Solar Eclipse in America.*

The solar eclipse of Aug. 27 was noticed here by a well-regulated time-keeper. At the commencement, the sun was transiently obscured by thin clouds. But the time here noted may be depended on as correct within a few seconds. At the termination, the clouds had dispersed, and the time was ascertained with precision.[*]

Beginning...... 8^{h} 1′ 30″ } Appar. time, a. m. *Portland Gazette.*
Ending........ 10 34 10 }

V. *Cashmere Goat.*

It appears from a memoir read before the French Royal Academy, that the Cashmere shawl made from the down of this animal is likely to become an article of European manufacture. Two foreigners, of the names of Jaubert and Terneaux, having introduced a number of these animals, they have much engaged the attention of the French naturalists, and it appears that their mode of treatment has been so far successful that out of a flock of 1229 goats purchased in Astracan there remains more than one third of the original number which produce the finest down.

VI. *Aphlogistic Lamp.*

It appears from a series of experiments made by Mr. Daniell, that the acid formed by the slow combustion of ether in this instrument is *acetic* combined with some compound of carbon and hydrogen, differing from ether. Most of our readers are acquainted with the form of this lamp, and we merely notice its construction to propose the occasional addition of a small tube similar to the chimney of an Argand lamp, which, on being held over the coil of platinum wire will immediately produce flame, although it had not previously exceeded a red heat. This takes place in consequence of the increased supply of oxygen furnished by the current of atmospheric air, the wire gradually passing from a red to a white heat, and from that to flame.

VII. *Medico-Chirurgical Society of Edinburgh.*

This Society, which promises to be in the highest degree useful, is formed upon the model of the London Society of that name. Most of the Medical Professors in the University, and many of the most respectable practitioners in the City, have co-operated in its formation. Dr. Duncan, sen. has been elected its first President. Its sittings commence in the approaching winter season.

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[*] The latitude of Portland is 43 39 32 N. and the estimated longitude (for it has never been ascertained by astronomical observation) is 70 12 W. from Greenwich.

VIII. *Lithography.*

An experiment has lately been made to take off impressions from the leaves of plants by lithographic printing. It appears to have been attempted by merely pressing the leaves against the stone. This process does not, however, appear the most adviseable, the better way being to cover the plant with the prepared ink, and after bringing a sheet of clean paper in contact with its entire surface, transfer the impression thus procured to the lithographic stone. We notice this from the great advantage which botanists are likely to derive from this simple mode of preserving and multiplying impressions from rare plants, which could otherwise only be seen in the cabinets of a few collectors.

IX. *Society for the Encouragement of Geography.*

A Society has been formed at Paris for the encouragement of geography, by the printing of scientific memoirs, the publication of charts, the distribution of prizes, and defraying the expenses of travellers having useful and important objects in view.

X. *Russian Voyage of Discovery.*

Accounts from Capt. Billinghausen, Commander in the Russian Voyage of Discovery in the Antarctic Seas, dated May, 1820, report that he had discovered three islands covered with snow and ice, on one of which was a volcano, lat. 56° S. He announces that there is no southern continent, or, should there be one, it must be inaccessible, from being covered with perpetual snows, ice, &c.

XI. *Racing Pedometer.*

An instrument has lately been invented in France which precisely marks the time that not only the winning, but every other horse takes in running the course, even if there should be 30 of them, and the interval between each only a quarter of a second. The "Jury of the Races," in the Arondissements of Paris have expressed their full approbation of the instrument.

XII. *Life Preserver.*

An experiment for saving lives from shipwreck, on Mr. Tregrouse's principle, which promises to be of great utility, has been tried with success in Yarmouth Roads, by Rear-Admiral Spranger. It consists in throwing, by a rocket, a line from the ship to the shore, and when the communication is established, binding to that a deep-sea line, or any of the running rigging. When these reach the shore, a larger rope, sufficiently strong to bear four men in a chair, is conveyed to the vessel, and the chair pulled on shore by means of a small rope, from whence it returns empty to the ship for a fresh cargo. On one occasion, the chair was on shore in five minutes after the firing of the rocket.

ARTICLE X.

NEW SCIENTIFIC BOOKS

PREPARING FOR PUBLICATION.

Dr. Reade is about to publish a Treatise on Vision, founded on numerous and interesting experiments.

Lectures on the Elements of Botany. Part I. with Plates. By Anthony Todd Thomson, FLS. Member of the Royal College of Surgeons, &c. &c. In 8vo. Vol. I.

A Synopsis of British Mollusca. By W. Elford Leach, MD.

A natural Arrangement of British Plants. By S. T. Gray.

JUST PUBLISHED.

The Physician's Guide. By Adam Dods, MD. 8vo. 10*s*. 6*d*.

A compendious Treatise on the Theory and Solution of cubic and biquadratic Equations, and of Equations of the higher Orders. By the Rev. B. Bridges, BD. FRS. Fellow of St. Peter's College, Cambridge. 6*s*.

Pomarium Britannicum, an Historical and Botanical Account of Fruits known in Great Britain. By Henry Philips. Second Edition. 8vo. 10*s*. 6*d*.

The Natural History of British Quadrupeds. By E. Donovan, FLS. Coloured Plates. 3 Vols. royal 8vo. 5*l*. 8*s*.

Illustrations of British Ornithology. Series First. Land Birds. By P. J. Selby, Esq. No. 2, elegant Folio. 1*l*. 11*s*. 6*d*. plain, or 5*l*. 5*s*. coloured after Nature. 12 Plates.

Illustrations of the Linnæan Genera of Insects. By W. Wood, FRS. &c. 2 Vols. royal 18mo. 1*l*. 10*s*.

ARTICLE XI.

NEW PATENTS.

John Manton, of Dover-street, Piccadilly, Middlesex, gunmaker, for an improvement in the construction of locks of all kinds of fowling-pieces and fire-arms.—July 30, 1821.

Thomas Bennet, jun. of Bewdley, Worcestershire, builder, for certain improvements in steam-engines or steam apparatus.—Aug. 4.

John Slater of Birmingham, manufacturer, for improvements in making a kitchen-range and apparatus for cooking, and other purposes.—Aug. 4.

William Henry Higman, of Bath, sadler, for certain improvements in the construction of harness.—Aug. 14.

David Gordon, Esq. of Edinburgh, now residing at Stravener, for certain improvements in the construction of wheeled carriages.—Aug. 14.

Jean Frederic, Marquis de Chabannes, of Russel-place, Fitzroy-square, Middlesex, for a new method and apparatus for attracting and catching fish.—Aug. 14.

Article XII.

METEOROLOGICAL TABLE.

1821.	Wind.	Barometer.		Thermometer.		Evap.	Rain.	Hygr. at 9 a.m.	
		Max.	Min.	Max.	Min.				
9th Mon.									
Sept. 1	N W	30·07	29·92	69	57	—	15		
2	S W	30·07	29·97	76	55	—			
3	S W	29·97	29·88	78	62	—			
4	S W	29·88	29·85	75	60	38	—		
5	W	29·95	29·85	72	57	—			☾
6	S E	29·99	29·70	76	62	—	—		
7	S E	29·70	29·70	77	56	—	11		
8	N W	29·74	29·70	69	50	43			
9	Var.	29·74	29·73	72	52	—	37		
10	W	30·00	29·73	70	50	—	11		
11	W	30·01	29·76	71	52	—	—		
12	S W	29·98	29·69	71	51	—	13		○
13	N W	30·01	29·98	66	51	47	15		
14	Var.	30·27	30·01	61	45	—	04		
15	N	30·27	30·24	69	54	—	—		
16	S W	30·24	30·16	74	59	—	—		
17	W	30·16	29·98	73	61	—			
18	N W	29·98	29·92	72	52	—			
19	N W	29·95	29·92	65	44	47			☽
20	S W	29·96	29·79	65	57	—	11		
21	S W	29·80	29·76	71	47	—	9[illegible]		
22	N E	29·80	29·75	70	53	—	01		
23	E	29·75	29·72	68	45	—	—		
24	W	30·04	29·74	66	44	—			
25	W	30·04	30·03	66	54	—			
26	S W	30·03	29·93	70	54	—	17		●
27	N W	29·95	29·93	63	49	—			
28	S W	29·93	29·53	65	52	—	30		
29	W	29·85	29·54	58	45	56			
30	N W	29·85	29·75	59	50	14	02		
		30·27	29·53	78	44	2·45	2·65		

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

REMARKS.

Ninth Month.—1. Rainy: a very heavy shower between five and six, p. m. 2, 3. Cloudy and fine at intervals. 4. Fine: showery. 5. Fine. 6. Fine: frequent lightning during the night, from half-past twelve to five. 7. Fine. 8. Fine: cloudy. 9. Fine, with showers: very wet evening. 10. Some thunder about noon: heavy showers: lunar halo. 11. Cloudy: fine. 12. Showery morning: cloudy. 13. Cloudy: rainy night. 14. Drizzling: a *Stratus* on the marshes at night. 15. Foggy morning: cloudy. 16. Gloomy morning: a shower in the evening. 17. Cloudy: fine. 18, 19. Fine. 20. Cloudy, with small rain. 21. Overcast: began to rain about eight, p. m. and continued till two next morning: a heavy thunder storm during the night. 22. Cloudy. 23. Gloomy. 24, 25. Overcast. 26. Showery. 27. Cloudy. 28. Fine: rainy night. 29. Cloudy: showers. 30. Cloudy: fine.

RESULTS.

Winds: N, 1; NE, 1; E, 1; SE, 2; SW, 9; W, 7; NW, 7; Var. 2.

Barometer:	Mean height	
	For the month	29·902 inches.
	For the lunar period, ending the 18th	29·951
	For 14 days, ending the 11th (moon south)	29·846
	For 14 days, ending the 25th (moon north)	29·982
Thermometer:	Mean height	
	For the month	60·950°
	For the lunar period	63·984
	For 31 days, the sun in Virgo	62·552
Evaporation		2·45 in.
Rain		2·65

Laboratory, Stratford, Tenth Month, 23, 1821. **R. HOWARD.**

ANNALS

OF

PHILOSOPHY.

DECEMBER, 1821.

Article I.

Biographical Sketch of the late John Rennie, Esq. FRS. and FAS. Lond. and Edin. &c. &c.

A few facts hastily collected form all the materials we have yet been able to procure relative to the early history and subsequent rapid progress of this distinguished individual; and these we offer to our readers trusting that other and more able pens will record the praise his works so justly merit.

John Rennie was born June 7, 1761, at Preston Kirk, in the county of East Lothian. His father, Mr. James Rennie, was a most extensive and respectable agriculturist, and died in the year 1766, leaving a widow and nine children, of whom John was the youngest. The future provision for, and care of, the family now devolved on his elder brother George, who, in conjunction with his mother, undertook the education of the younger children, and in due time young Rennie was sent to school at Preston. Here he acquired the first rudiments of his education, though it does not appear that he made any very remarkable progress at this seminary. Immediately adjoining Mr. Rennie's estate, and between the farm and school, was a small river, and it was necessary for young Rennie to cross this several times in the day. The passage of the river was usually accomplished by means of a rustic bridge of stepping stones, and when it was swollen, the only alternative was a boat kept by Mr. Meikle for that purpose. This gentleman was at that time considered one of the most ingenious millwrights in Scotland, and he afterwards distinguished himself by the invention of the thrashing machine, and effected considerable improvements in the construction of water-wheels, the latter of which he brought to great perfection.

It will readily be imagined that no better school than Mr. M.'s workshop could have been found for ripening into perfection those seeds of science with which nature had endowed the young mechanic; indeed it is more than probable that to the opportunities thus obtained we may attribute his future advancement in life. He would watch with delight the different operations that were carrying on, and he soon acquired confidence enough to mingle in the labours of the workmen. His leisure time was spent in the construction of such models as came under his observation, and at 10 years of age he had contrived to make models of a steam-engine, a windmill, and a pile engine, upon Valoue's principle. At 12 years of age he disagreed with his schoolmaster, whom he thought incompetent to teach him any longer, and immediately left the school. At a loss how to employ himself, he expressed a wish to be placed under his friend Mr. Meikle, with whom he continued about two years, when finding himself deficient in some of the more essential parts of general education, he proposed going to Mr. Gibson, an able teacher then resident at Dunbar. Here he distinguished himself in a remarkable manner by the assiduity and talent he displayed in the acquirement of mathematical knowledge; and on Mr. Gibson being appointed Master of the Public Academy at Perth, he earnestly recommended John to succeed him in the management of the school. He soon, however, returned home, and remained with Mr. Meikle for some time, occasionally employing himself in the drawing and construction of machinery. About this period he undertook the repairs of a corn mill situate in his native village, and on its completion went to Edinburgh. He was then about 17 years of age, full of ardour, and anxious to improve himself, without, however, neglecting the honourable employment of those talents he had already so well cultivated. Accordingly we find him in the summer of 1778 busily employed in the erection of a flour mill in the county of Angus, and this was followed in the succeeding year by the completion of another at Kirkaldy, and a third for Mr. Atchison, of Drummore, the intermediate time being spent at the college at Edinburgh.

The good fortune, or rather the indefatigable industry, of young Rennie, soon surmounted all the difficulties that opposed the completion of his collegiate studies, and he was shortly noticed by Professor Robison as a young man of extraordinary genius and application, and by him recommended to Messrs. Boulton and Watt as a fit person to be employed in the construction of mill-work.

About the same period, Messrs. Boulton and Watt began to apply the steam-engine to a variety of purposes unconnected with the raising of coals and water (its original object), and selected Mr. Rennie as a fit person to superintend the performance of the Albion Mills, which was then just completed. His salary, though fully equal to his wants, was at first but small, and this afforded an additional stimulus to exertion; so that at the completion of the Albion Mills he was engaged to superintend the erection of some extensive machinery at Messrs. Whitbread's brewhouse, and an opening was thus presented for him to commence business on his own account.

About this time, Mr. Smeaton, the celebrated engineer, died, and left a chasm in that department of science; and a more favourable combination of circumstances for Mr. Rennie's establishment could not have presented itself. A new power for moving machines had just then been invented, and Mr. Rennie was protected by, and connected with, the inventor and patentee. From the year 1794 to the day of his death, Mr. Rennie was at the head of the list of civil engineers, and became connected with every undertaking of magnitude. Among his first essays may be enumerated Crinan Canal, in Scotland, and the Lancaster Canal, the former remarkable for the numerous practical difficulties which occurred throughout the whole of the execution; the latter, for the aqueduct over the river Lime, one of the largest of its kind in Europe. To enter into a detailed account of his numerous works would be endless: they are before the public, who are capable of appreciating their merits; suffice it to say, that he executed with success, the Kennet and Avon, Buchan, and Aberdeen Canals, the Harbours of Frasersburgh, Queen's Ferry, Berwick, Howth, Holyhead, Dunleary, &c.; the London, East India, Hull, Leith, Liverpool, and Dublin Docks; the Breakwater, Plymouth; the Royal Dockyards of Sheerness, Pembroke; the Bridges of Kelso, Musselburgh, Newton Stewart, Waterloo, and Southwark, besides an infinity of others. He was among the first who perfected the Diving Bell, and rendered it entirely subservient to the purposes of building under water.

He was Fellow of the Royal Societies of London and Edinburgh, the Antiquarian Society, the Geological Society, the Royal Irish Academy, and the Royal Society of Munich, besides belonging to various other minor institutions for the promotion of scientific knowledge. In the execution of the public works which he superintended, there were spent more than thirty millions sterling.

The list that we have now furnished forms but a small portion of the works which have emanated from the designs of this distinguished engineer; indeed his industry is almost without parallel; and on going to France for a short time in 1816, he is said to have stated, that it was the first relaxation he had taken for nearly 30 years. His habits of business were very early, and he frequently made appointments by day-break in the morning, continuing closely employed till eight or nine o'clock at night.

At the age of 25, Mr. Rennie married a Miss Mackintosh, by whom he had nine children, six of whom are now living, and it is probable that the eldest of these gentlemen will succeed to his father's professional employment.

M. Dupin, who is so well qualified to do justice to the merits of the late Mr. Rennie, has, in a Notice Necrologique respecting him, addressed to the Royal Institute of France, paid a tribute to the virtues and amiable qualities of that distinguished individual, and given a brief but masterly account of his principal works.

" Mr. Rennie," says M. Dupin, " raised himself by his merit alone. In a country in which education is general, he received from his infancy the benefit of instruction, which he afterwards knew how to appreciate.

" Scotland has the glory of having produced most of the civil engineers who, for nearly a century, have executed the finest monuments of the three kingdoms, and the most ingenious machines; James Watt, John Rennie, Thomas Telford, &c. seconded with so much ability by the Nimmos, the Jardines, the Stevensons, &c."

After enumerating the works executed by Mr. Rennie for Messrs. Watt and Bolton, and his application of steam to machinery for clearing canals, he observes:

" Mr. Rennie learned immediately from Smeaton the art of directing hydraulical constructions; he formed himself by the counsels and example of that great engineer, and by the study of the works of a master whom he was to equal in some respects, and surpass in many others."

M. Dupin then alludes to the East India and London Docks, and the completion of the West India Docks, and observes:

" At the very moment he was snatched from us by death, he was busied in finishing a new construction equally ingenious for its architecture and its mechanism. Vast roofs, supported by lofty columns of cast iron, present in the middle of their structure aerial roads, on which are made to run carriages, whose mechanism is so contrived, that by their means, enormous mahogany trees kept in these fine magazines, may be raised and let down at pleasure. By means of this ingenious system, a few workmen now execute in a few minutes what required formerly whole hours, and a number of workmen."

This candid and liberal-minded foreigner concludes his *eloge* with the following striking reflections on the new character which has been given to the erections of this country by Mr. Rennie:

" If, from the incalculable effect of the revolutions which empires undergo, the nations of a future age should inquire what was formerly the New Sidon, and what has become of the Tyre of the West, which covered with her vessels every sea? Most of the edifices, devoured by a destructive climate, will no longer exist to answer the curiosity of man by the voice of monuments; but the bridge built by Rennie in the centre of the commercial world, will subsist to tell the most distant generations here was a rich, industrious, and powerful city. The traveller, on beholding this superb monument, will suppose that some great Prince wished, by many years of labour, to consecrate for ever the glory of his life by this imposing structure. But if tradition instruct the traveller that six years sufficed for the undertaking and finishing of this work; if he learns that an association of a number of private individuals was rich enough to defray the expense of this colossal monument, worthy of Sesostrises or Cæsars, he will admire still more the nation in which similar undertakings could be the fruit of the efforts of a few obscure individuals lost in the crowd of industrious citizens."

Mr. Rennie died at his house in Stamford-street on Oct. 4, in the 64th year of his age, after a very short illness; and his remains were interred on the 16th in St. Paul's Cathedral, near those of Wren, Barry, Milne, Reynolds, and West.

Among a vast number of distinguished persons who followed Mr. Rennie to the grave, were, Sir Joseph Yorke, Sir Humphry Davy, Sir R. Seppings, Sir G. Cockburn, Sir J. B. Martin, Sir Thomas Lawrance, Mr. Chantrey, Mr. W. T. Brande, and several other gentlemen of rank and high professional abilities. The funeral arrangements were conducted without any affectation of splendour, but extremely handsome and well devised; and in the long train of mourners were many to whom his exertions had been valuable, and many by whom his talents had been admired.

ARTICLE II.

On Floetz Formations. By Thomas Weaver, Esq. MRIA. MRDS. MWS. MGS.

(*Concluded from* p. 359.)

FLOETZ.—SERIES III.

1. *Upper or Shell Limestone Formation of M. Freiesleben.*

Shell Limestone. Muschel-kalkstein of Werner.

Lias and Oolite Limestone Formation.

This formation is also very widely distributed. In Mansfeld, it is commonly disposed in regular horizontal strata, which sometimes present a smooth even sheet of considerable extent;

but they are often also rapidly inclined, or gently undulated, and, generally speaking, in a position unconformable to the older formations, upon which they repose; appearing sometimes also in the form of isolated caps or subconical hills. In other districts, the relations are similar. Thus in the tract between the forest of Thuringia and the Rhön, M. Heim describes the shell limestone as being there generally horizontal, to the top of the highest hills; but in some parts, the strata are variously inflected, and elevated even in the vertical position. The mean height of the shell limestone in the forest of Thuringia is estimated at 1000 feet, but in the Geba mountain, it reaches to 1300 feet.

Near the surface, the strata of shell limestone vary between half an inch and four inches in thickness; but at a greater depth, they acquire a thickness of six or eight feet. They are frequently in a disjointed state, being traversed by fissures, nearly vertical, mostly open, and sometimes several feet in width.

The shell limestone is upon the whole distinguished by its homogeneous character, its grey, yellow, and white colours, and by the abundant remains of organized bodies which it contains, mostly assembled in families. With the exception of a few layers of sandstone, hornstone, or flint, it is in a great measure free from siliceous matter.

Its most remarkable varieties arise from an intermixture of silex, alumine, calcareous spar, and oxide of iron, or accordingly as it contains, or is destitute of, petrifactions. Some of these varieties pass into sandstone, and others into marl.

In Mansfeld, and the adjoining parts of Thuringia and Anhalt, the pure limestone is commonly yellowish-grey, isabella-yellow, or yellowish-white; more rarely smoke or bluish-grey; fracture, generally fine splintery, or even, and flat conchoidal, and dull, and, more rarely, granularly foliated in part. It is seldom of a dark bluish-grey colour, approaching to black, in which case it forms a black stripe in the middle of a stratum, the colour becoming lighter and fainter toward the exterior. But some strata are almost of a hornstone character.*

Among the principal varieties of the shell limestone may be noticed the following:

a. Mountain-green, or greenish-grey limestone, of a soft tender, fine sandy consistence;

b. Ochre-yellow limestone, tender, feeling like sandstone;

c. Foliated granular yellow limestone;

d. Ochre-yellow, dull, fine sandy limestone, with local assemblages of calcareous spar;

e. Porous limestone, resembling tuffstone, compounded of the fragments of shells, the substance of which is mostly yellowish-grey and foliated, cemented by an ochre-yellow sandy medium.

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* The preceding description accords very well with the general characters of the English lias limestone.

The yellow varieties are usually more clayey, or sandy and tender, than the grey homogenous kinds; but some of them are partly of a magnesian quality, locally known by the name of *mehlbatz.* They all, however, distinctly alternate with each other, in beds of greater or less thickness.

Sometimes the dark-grey compact argillaceous limestone contains fragments of compact limestone, of various sizes, then acquiring a spotted appearance.

The compact shell limestone of the Seeberg, near Gotha, is partly oolitic; small compact round and oval grains, of a dark-grey colour, being immersed in a yellowish-grey base, and forming a stratum two feet thick.*

Cylindrical channels, from one-fourth to half an inch in diameter, several inches in length, and closed at both extremities, are very common in the shell limestone, particularly in that which is pure and compact. These channels are straight, or they wind in various directions, the sides being smooth; and solid cylinders of a similar form, separating easily with a smooth surface, are also common. Their origin is unknown.

In most districts, one or more layers of flint, hornstone, or a compound of the two, may be met with in the shell limestone; but they rarely exceed a few inches in thickness, and are seldom continuous for any great distance. Sometimes also these substances appear in the form of nodules.† And in Henneberg are found in the shell limestone several layers of yellow and brown siliceous limestone, which in some parts pass into distinct yellow and brown jasper, and in others into siliceous brown ironstone.

White calcareous spar, in rather large masses and druses, and in disseminated grains, appears also sometimes in the shell limestone; and stalactiform and botriform calcsinter is found incidentally between the strata.

Slaty and earthy aphrite have occurred in it near Polleben, in Mansfeld, and agaric mineral near Asse, in Wolfenbüttel.

Compact and fibrous gypsum, in layers extending nearly to two feet in thickness, have been found, it is said, in the shell limestone, near Naumburg.‡ Potter's clay occurs in it near Freyburg, on the Unstrutt.

Organic Remains.—The pure and marly beds of the shell limestone appear to contain a greater quantity of petrifactions than those which are sandy. The remains of bivalve and univalve shells are numberless; sometimes forming a congeries of shells in layers from a half to one inch thick between the strata; while others occur in the substance of the limestone.

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* The oolitic structure appears in a few instances also in the English lias limestone.

† These substances occur under the same forms in the English lias limestone.

‡ Gypsum is met with also in the shell limestone formation of England, in groups No. 12, 10, and 9, of Mr. Greenough's Geological Map.

Remains of fish, and ribs, vertebræ, bones, and teeth of unknown quadrupeds,* are less common. In several parts of Thuringia, however, a marly limestone, of a thick slaty texture, is found immediately above the upper gypsum, containing numerous petrified bones, closely imbedded in the mass; beside teeth of fishes, large smooth terebratulites, echinites, and pieces of wood coal.† These bones are sometimes converted into an opaline mass, while the cavities are lined with crystals of quartz. Large rib and blade bones have also been met with, the true character of which has not been ascertained. Fragments of bones occur singly in most of the strata of the shell limestone. And numerous teeth of fishes, of various sizes, and of different species, occur also in the shell limestone of Thuringia, beside single scales, bones, and vertebræ, of fish.

According to M. Blumenbach an undoubted ornitholite has been found in the shell limestone of the Heinberg, near Göttingen.‡

Entrochites generally appear in the lowest strata of the shell limestone, closely crowded together in limestone resembling hornstone, and which is mostly composed of the fragments of encrinites. Sometimes whole families of encrinites are thus found in the same vicinity, and partly in a good state of preservation.

Belemnites, gryphites, buccardites, venulites, and a peculiar species of pentacrinite, are confined to particular districts.

Echinites, spines of echinites, dentalites, patellites, trochilites, and striated terebratulites, are very rare.

But other well preserved terebratulites, ostracites, buccinites, pectinites, strombites, turbinites, chamites, ammonites, mytilites, myacites, &c. are endless, beside whole strata composed of the fragments of shells, which are no longer recognizable. Discites and asteriacites are more rare. Beside these, there have been observed also trigonellites, donacites, nautilites, tellinites, and pectunculites.

Baron von Schlotheim has observed also serpulites, helicites, neritites, conilites, solenites, and lepadites.§

Some of the bivalve and univalve shells still preserve their natural nacrous lustre, and in particular the terebratulites; others are converted into compact limestone or calcareous spar, or into conchoidal hornstone, and more rarely into sparry iron ore; and still more rarely they consist of brown iron ochre, which, falling out, leaves the empty impression of the shell.*

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* Probably allied to the *Enalio-Sauri* order of animals, to the monitor, or to crocodiles, of which, as occurring in the lias, oolite, green sand, and chalk series of England, an admirable account has appeared, from the joint researches of M. de la Beche and the Rev. W. D. Conybeare, while this paper is passing through the press.—(See Geological Trans. vol. v. part ii.)

† An analogous bone bed is found in the lower part of the lias limestone, near Wickwar, and in Westbury and Aust cliffs, on the banks of the Severn, Gloucestershire; so also on the Somersetshire coast, near Watchet.

‡ Remains of birds are stated by Mr. Greenough to have occurred in the Stonesfield slate of the oolite series in England.—(See Geol. Essays, p. 300.)

§ Petrefactenkunde.

Coal.—Traces of coal appear only in the upper beds of the shell limestone formation, but they are met with in many places; generally consisting of a stratum of black clay, which includes thin discontinuous layers, stripes, or laminæ of slate coal; the mass being commonly much contaminated with iron pyrites, and hence it soon disintegrates on exposure to the air, and forms with water a viscid paste. From this circumstance, it has been called *clay-coal* (letten-kohl) by M. Voigt. The bed, however, is frequently composed for considerable distances of alum-shale rather than coal, and, when burned, leaves a residuum of white slaty clay. The coal is seldom fit for the forge, though a portion of pitch coal has sometimes appeared in these beds, which have been wrought in various quarters, at different periods, partly with a view to coal, to vitriol, or to alum; often, however, without advantage. The clay, in which the coal lies, contains remarkable impressions of plants, seed vessels, and seeds, which are referred by B. von Schlotheim to unknown species of trees. As an example of this kind of coal may be mentioned, the coal near Kutzleben, in Thuringia, where one bed, 10½ inches thick, composed of black clay, with single thin stripes of coal, and covered with greenish-grey marl, reposes upon the shell limestone; beneath which, at the depth of 14 fathoms, is a second coaly bed in the shell limestone, from 10 to 16 inches thick. This contains smiths' coal, beside pitch coal two or three inches thick. The carbonaceous layers have varied, in different quarters, from 8 to 21 inches in thickness.†

In tracing the extent of the shell limestone, M. Freiesleben follows that formation into Thuringia, the Hartz, Lower Saxony, the forest of Thuringia, Franconia, Suabia, France, the heights of Jura, the Alps, Dalmatia, &c. partly relying on the authorities of MM. Heim, Voigt, Hausmann, Flurl, &c. And as a corroboration of part of that view, the remarks of MM. Ebel and von Raumer may also be noticed; the former of whom observes that beds of oolite are found throughout the Jura chains, the grains of which vary from the size of lentils to that of peas, and even larger; ‡ and the latter remarks, that the newest shell limestone is rich in beds of oolite, giving as instances the oolite near Rumigny, at the south-western foot of the Ardennes, and the oolite of the Jura.* The first notice of the latter, however, is, I believe, due to M. de Saussure.†

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* I have seen most of the analogues of the organic remains noticed above in the shell limestone of Gloucestershire, beside others not included in that list; e. g. in the lias limestone, pinnites, plagiostomites, crabs, prawns, shrimps, spines of balistæ; and in the oolite, corallites, orthoceratites, plagiostomites, volutites, pinnites, arcacites, cucullæites, astarte, mactra gibbosa. (Sowerby). Several of the remains occurring in the beds of the lias and oolite series are to be found depicted and described in M. Sowerby's Mineral Conchology.

† As an analogous formation in England, we may refer to the bad coal found in the oolite of the eastern Moorlands of Yorkshire, in group No. 13 of Mr. Greenough's Geological Map; and also to the bituminous slate clay, called Kimmeridge coal, in group No. 10 of the same Map.

‡ Ebel. Alpenbau, vol. ii. p. 109.

The general character and distribution of the shell limestone formation in England, as consisting of the lias and oolite series, with their accompanying beds of clay, marl, and wood coal, may be learned from Mr. Greenough's Geological Map, comprehending the groups No. 15 up to No. 9 inclusive. And several illustrative facts may be collected from Mr. Webster's observations in Sir H. Englefield's splendid work on the Isle of Wight, &c. and from Mr. W. Phillips's Compendium of the Geology of England and Wales.

2. *Quader and Pläner Sandstein and Kalkstein.*

Third Floetz Sandstone Formation of Werner.

Ferruginous and Green Sandstone and Limestone Formation.

In my notices of this formation, I shall confine myself chiefly to the observations of MM. Hausmann, von Schlotheim, and von Raumer.

1. According to M. Haussmann, the quadersandstone constitutes in Lower Saxony a chain of low hills and eminences, which commencing at the north-eastern foot of the Hartz, between Quedlinburg and Blankenburg, proceeds in a north-western direction as far as into Westphalia. It consists of a uniformly fine-grained sandstone, usually of a white colour, composed simply of grains of quartz, with a very slight proportion of cement, often scarcely perceptible. The cement is commonly argillaceous, and frequently more or less iron-shot, so that the white ground is stained with stripes or spots, of a yellow, red, or brown colour; or the whole substance is thus discoloured. The iron-shot cement passes sometimes into argillaceous ironstone; and the argillaceous cement is casually penetrated with bituminous matter, then assuming a grey or black colour. The connecting medium sometimes consists also of clay marl, or calcareous marl; and more rarely of quartz, or calcedony. Strings of quartz traverse the sandstone occasionally, forming cells when the substance of the rock is decomposed. It is always stratified, in strata from half a foot to several ells in thickness; but never slaty, like the new red or variegated sandstone. It is further distinguished from the latter by the following characters: it is firmer and more compact, with a smaller proportion of cement, and is less discoloured; it contains mica much more sparingly,

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* See the appendix to the work of M. von Raumer, quoted above, in which the author offers some remarks on the analogy of the newer floetz formations in England, France, Germany, and the Alps.

† Voyages dans les Alpes, sect. 358.

which is distributed through the mass in single scales, and its planes of separation are never covered with mica; it is free from clay galls, and from drusy cavities, occupied by crystals of calcareous spar or quartz; and it contains seams of coal, which have hitherto been sought for in vain in the new red or variegated sandstone.

It is also to be observed that when the quadersandstone and the new red sandstone occur in the vicinity of each other, the former is always found above the latter, and never alternating with it; generally speaking, however, the shell limestone is interposed between the two.

In Westphalia, green sandstone appears near Unna, Werl, and Soest, according to M. Hövel; its substance being composed of a medium between marl and sandstone, and containing green particles.

In the quadersandstone of Lower Saxony, M. Haussmann notices the occurrence of three distinct ferruginous, or ironstone beds: The lowest consists of grey or black clay, which contains a stratum 3½ feet thick, composed of round masses of indurated ironshot clay, which are surrounded with a coating of clay ironstone, in the compact or ochry state; and accompanied with pieces of petrified wood and of pitch coal, and by ammonites, belemnites, and other organic remains.

The intermediate bed contains brown compact clay ironstone, disposed in thin concentric laminæ, or mixed with minute sand and scales of mica; brown iron ochre also occurs; the whole frequently forming layers one above another, and in the aggregate from one and a half to seven feet thick, e. g. at the Fuhregge in the Weser district.

The most recent, or highest, bed in the formation is from 7 to 14 feet thick, consisting of iron-shot soft clay with grains of quartz, and bearing lenticularly granular clay ironstone, accompanied by scales of mica, and a leek green substance resembling steatite. Clay forms both the roof and floor of this bed.

It is not improbable that the lenticular clay ironstone of the South of Germany, and other countries, may belong to a formation of the same era.

The seams of coal, which occur in the quadersandstone of Lower Saxony, lie lower down, and deeper, in the series than the ferruginous beds just noticed. They vary from 8 to 12 inches in thickness, and repose on slate clay free from impressions of vegetables, and which passes into clay marl, succeeded by sandy marl, sandstone, or limestone. The coal beds consist of coarse coal, which passes into slate coal.

The thin coaly seams in the quadersandstone near Quedlinburg and Blankenburg, consist of soft clay containing slight layers of coal. These are interposed between beds of slate clay, free from vegetable impressions, which have sandstone both for the roof and floor. In the mine Glück-Auf are three beds of this description, the uppermost of which is found at the depth of 18 fathoms from the surface, being only five inches thick; the middle one at the depth of 28 fathoms, being 18 inches thick, and containing some useful coal; but the lowest bed contains the best coal, which is a kind of pitch coal approaching to moor coal.

Organic Remains.—Numerous remains of bivalve and univalve shells, partly well preserved, partly broken, are found in the quadersandstone of the Blatenberg and Heidelberg, near Blankenburg. Their substance consists not unfrequently of calcedony, connected with a calcedonic mass.* Petrified remains of plants are also numerous, appearing near Blankenburg, in the form of large leaves, which have some resemblance to those of oak, lime, and fig trees, but are much larger, and also to those of the palm tribe, according to B. von Schlotheim; and near Lauchstädt whole strata are composed of petrified leaves, with stems and branches of wood, in such quantity as to resemble the remains of a fallen forest.† Fragments of vegetables, composed of brown coal, occur near Goslar and Göttingen. Petrified fragments of bones are also found in the quadersandstone. M. von Schlotheim observes, that, upon the whole, the same petrifactions occur in the newer floetz fine-grained sandstone or quadersandstone, that are met with in the shell limestone, although much more sparingly; the sandstone being sometimes wholly free from them. Pinnites, turbinites, pholadites, pectinites, buccardites, ostracites, tellinites, mytilites, and myacites, seem of most common occurrence; but there are also noticed, pectunculites, chamites, venulites, trigonellites, donacites, terebratulites, serpulites, dentalites, muricites, buccinites, bullacites, strombites, volutites, conilites, nautilites, asteriacites, corallites, more rarely echinites, and, scarcest of all, encrinites.

2. In the tracts considered by M. von Raumer, in his work referred to above, this formation occupies two principal districts; one being situated on the south-west of the Eulengebirge, and the other on the north of the Riesengebirge. These deposits belong to those widely extended masses, which are unequally spread over the northern parts of Germany, occupying portions of Moravia, Bohemia, Silesia, Lusatia, the Ertzgebirge, Lower Saxony, and Westphalia, and which in the course of their extent come in contact with, and cover rocks of very different eras; being generally disposed in an horizontal position, or slightly inclined.

The southern deposit reposes partly on primary rocks, partly on the old red sandstone tract, described in a former part of this paper. The beds of the formation consist of:

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* The silicified state of many of the organic remains found in the green sand in England is a well-known fact, calcedonic masses also appearing.

† In the quaderstandstone near Gotha, M. von Schlotheim observed palmacites canaliculatus, p. obsoletus. (See Petrefactenkunde.)

1. Quadersandstone, a yellowish and greyish-white fine grained sandstone.

2. Plane sandstone, a yellowish, brownish, or ash-grey sandy marl.

3. Plane marl, a pure calcareous marl.

4. Plane limestone, a grey fine-grained rock.

5. Slaty clay marl and clay, micaceous, and of greenish, yellowish, and ash-grey colours.

6. Grey coarse sandstone, and conglomerate.

These substances may be traced passing successively one into another. The clay marl is sometimes less slaty, and formed of pieces loosely cohering. In this state, it frequently contains balls of clay ironstone, composed of concentric lamellar distinct concretions.

Minutely disseminated green earth is very common in the plane marl; and sometimes also in the quadersandstone, in greater or smaller quantity.

The beds of this formation alternating and passing frequently into each other, it can only be loosely stated in what quarter particular beds predominate. In the southern portion of the district, clay marl and clay prevail; in the south-eastern and eastern, plane sandstone, clay marl, and clay; west of these is the principal chain of quadersandstone, in which plane sandstone is subordinate; and beyond this, plane limestone predominates, the quadersandstone appearing only in subordinate beds.

The animal remains noticed in this tract are shells, apparently belonging to the genera cerithium, turritella, and turbo; also solenites, pectinites, tellinites, venulites, terebratulites, and vermiculites; an echinite and shark's tooth have likewise been observed. Of vegetable remains, Baron von Buch remarked petrified leaves of trees, intermingled with shells, in the conglomerate of Kieslingswald; and M. von Raumer noticed a reed-like form in plane sandstone near Hundorf, and cylindrical forms, apparently of vegetable origin, in several places, in the quadersandstone, and in the grey clay marl. Several of these remains are found in the most dissimilar beds of the formation, e. g. pectinites in quadersandstone, and in plane marl; venulites in quadersandstone, and in the limestone which alternates with the grey conglomerate of Kieslingswald; and cylindrical tubes in quadersandstone, and in grey clay marl. This circumstance, the alternation of these beds, and their reciprocal mineralogical affinities, show that they all belong to the same formation.

The northern deposit of this formation reposes principally on the old red sandstone of that quarter, but partly also on a primary slaty tract. It has hitherto been but slightly examined. Quadersandstone appears to predominate, plane sandstone being rare; the former contains beds of clay, and some coal. Thus thick beds of clay appear in the quadersandstone, in the vicinity of Bunzlau, in which bituminous wood is found; and near Wenig Rackwitz, they contain pitch coal. In some quarters also, white sand is found in this formation. Cylindrical petrifactions, resembling those of the southern tract, are met with, beside pectinites. But both the northern and southern quader-sandstone tracts require to be strictly examined with respect to organic remains.

In extending his views of this formation to other tracts, M. von Raumer notices the observations of M. Flurl on the Alps of Upper Bavaria, in which sandstone and marl, coloured green by chlorite, are found containing nummulites in great quantity. And as nummulites are found also in the green sandstone of England and France, the identity of those formations is thence inferred. M. Ebel likewise states, that the beds of green sandstone in the most northern chain of the calcareous Alps, contain nummulites very frequently, beside other organic remains, e. g. in the Aubriggs, Zindeln, Fläsch, &c.[*]

Of the extent and character of the ferruginous and green sandstone and limestone formation in England, including the beds of clay, marl, and wood coal, associated in that series, a general idea may be obtained from Mr. Greenough's Geological Map, comprising the groups No. 8 up to No. 6 inclusive; and various facts in elucidation of the formation may be gathered from the observations of Mr. Webster in the Geological Transactions, and in Sir H. Englefield's work on the Isle of Wight; from Mr. W. Phillips's Geology of England and Wales; and from Mr. Sowerby's Mineral Conchology. But a still more ample view of the relations of that series may probably be soon expected from the work of Mr. Mantell on the Geology and Fossils of Sussex, of which the prospectus has recently appeared.[†]

3. *Chalk.*—(*Kreide.*)

Upon this formation I do not think it necessary to offer any remarks. Its principal relations in England have been developed by Mr. Webster; and additional light will doubtless be thrown on the subject by the extended researches of Mr. Mantell. A general list of organic remains found in chalk has been given by Professor Jameson in his instructive notes on Cuvier's Theory of the Earth.

FLOETZ.—SERIES IV.

Nor is it my intention at present to enter upon the consideration of the fourth floetz series, which would require a more ample space than the limits of this paper admit. I shall merely observe in reference to that series, that, with the exception of the newest floetz trap formation of Werner, the chief merit in unfolding its relations rests in France with Baron Cuvier and M. Brongniart, and in England with Mr. Webster and Professor Buckland.

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[*] Alpenbau, vol. i. p. 372.

[†] According to that prospectus, the work will embrace the ferruginous sand; weald clay, with subordinate beds of sandstone and limestone; green, grey, and white sand; blue clay or galt; chalk marl, lower chalk, and upper chalk; plastic clay formation; and alluvium. Some very valuable preliminary notices of the organic remains contained in those formations, were published by that author in the Sussex Provincial Magazine for the year 1819.

In conclusion, I must add, that the principal object of the foregoing pages has been to remove misconception, to reconcile seeming discrepancies, and to simplify general views. How far that task has been accomplished, the geological inquirer will determine. He will also not fail to remark, that in the view which I have taken, the correspondence between the British and Continental formations, in their principal characters and in their general order of succession, is no less striking, than it is consistent in the main with the great outlines of Werner's arrangement —a coincidence so remarkable, as to warrant the inference that the general view entertained by that naturalist of the mineral structure of the globe was founded upon solid considerations. The evidence too leading to that conclusion is so much the more impartial, as the exemplifications of the chief positions are in a great measure drawn from the Geological Map of England and Wales; a work, avowedly constructed by its author with the intention of simply expressing facts, independent of all theory or system.

ARTICLE III.

On Crystallized Magnesian Carbonate of Lime from Alston Moor, in Cumberland, crystallized Plumbago, and some other Minerals from the Mines of Cumberland. By Edward Daniel Clarke, LLD. Professor of Mineralogy in the University of Cambridge, Member of the Royal Academy of Sciences at Berlin, &c.

(To the Editor of the *Annals of Philosophy*.)

DEAR SIR, *Cambridge, Nov. 1, 1821.*

There is great pleasure in being able, through the medium of your *Annals*, to establish an intercourse with mineralogists, however widely dispersed, and by communicating to them, from time to time, such facts as appear to be worthy of their notice, to invite them to that commerce of intelligence which is of the highest advantage in carrying on their favourite studies. The article which I sent to you upon *arragonite* gave rise to some observations from Dr. Daubeny respecting the discovery, by Prof. Stromeyer, of strontian in *all* the varieties of that mineral; with the exception only of the coralloidal variety called "flos ferri."[*] For this communication from Dr. Daubeny, many of us are indebted; because it was rather generally believed that strontian only existed in some of the varieties of arragonite; and I shall now be thankful to any of your correspondents who will point out a plain and simple process by which the presence and proportion of strontian, as a constituent of arragonite, may be clearly and accurately determined. The plan I have myself pursued had been considered as satisfactory; but there are some objections to it which will be obvious to chemists; it was merely that of dissolving the arragonite in dilute muriatic acid, adding sulphuric acid, and trying the sulphate thus formed, by nitric acid; which is supposed to have no action upon the sulphate of strontian, although it dissolve the sulphate of lime. For the present, therefore, I will dismiss this part of the subject altogether, and proceed to a few remarks upon other minerals perhaps not less likely to interest your mineralogical readers.

I believe that fine rhomboidal crystals of the *magnesian carbonate of lime* are rather rare. A dealer in minerals, who makes occasional visits to this University and to Oxford, for the sale of specimens found in the mines of Cumberland, brought lately to my house what he called "cubic carbonate of lime," from Alston Moor. Perceiving that the crystals were rhombic, I examined the supplementary angle of a minute fragment by Dr. Wollaston's reflecting goniometer, and finding that it measured 73° 45′, exposed this atom, which did not weigh the thirtieth part of a grain, to the test for exhibiting magnesia, which the same illustrious chemist, the inventor of that goniometer, has himself pointed out; namely, by dissolving it in muriatic acid in a watch glass, precipitating the lime by carbonate of ammonia, and then adding phosphate of soda, and drawing lines upon the glass with a glass rod. The process is now well known to chemists. I mention it only because all mineralogists are not aware of the extreme subtlety of Dr. Wollaston's test; it is such that a portion of magnesia, indefinitely small as to its quantity, existing in a fragment of magnesian limestone almost invisible to the naked eye, may yet be rendered strikingly conspicuous by this process; because the binary compound which is precipitated, or, as it is called, "the triple salt," consisting of the phosphoric acid united with magnesia and ammonia, appears in white streaks upon the glass in all parts over which the glass rod has passed. This was the case in the present instance; and as the crystals to which I allude measure half an inch in their major diameters, and may be had of all the Alston Moor dealers, the information will, perhaps, be thought desirable. Each crystal consists of a visible aggregation of minute primary rhombi, with a white opaque appearance, and a pearly lustre, seated upon small diaphanous crystals of quartz. Their other characters being those common to the magnesian carbonates of lime, need not be specified.

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[*] See *Annals of Philosophy* for Sept. 1821, p. 230.

In the hands of the same dealer I also observed, although not for the first time in my life, a specimen of the Carburet of iron with polyhedral surfaces, and possibly a result of crystallization. As this substance is, I believe, not known to mineralogists as a crystallized body, I shall be particular in describing its form. The specimen came from Keswick, and, of course, originally from the Borrowdale mine of plumbago. It was offered to me as the substance which the miners call the "leader to the plumbago," the carburet of iron not being in a pure mass, but in somewhat of a granular form, occupying cavities in white sparry carbonate of lime. In several instances, the white spar encrusting the detached portions of plumbago exhibited an inclination of plane surfaces meeting at an angle, by the common goniometer, of 118°, the plumbago itself corresponding in the inclination of its planes with the exterior coating. Some years ago, the Rev. Dr. Satterthwaite, of Jesus College in this University, visiting the Borrowdale mine (in answer to my inquiries after a regularity of structure in this remarkable mineral) obtained for me from the Director of the Works, a single specimen, exhibiting the same form with the same inclination of the plane surfaces. By comparing both together, I now find great reason to believe in the existence of crystallized plumbago, and the crystals, supposing those to be crystals which appear upon the specimens in my possession, may be described as oblique four-sided prisms with rhombic bases; the obtuse angle of the rhombic base measuring 118°.

A *catalogue raisonné* of the minerals found in the Cumberland mines would form a very interesting addition to our stock of mineralogical information. I shall just notice a few other substances on account of their extraordinary beauty. Among them are varieties of arragonite hard enough to make a deep incision in glass; crystals of quartz so penetrated by chlorite as to be quite opaque, and of a fine emerald green colour; the most magnificent cubic crystals of green fluor spar highly transparent, containing air bubbles moveable in a liquid which is believed to be water, but this has not been ascertained; stalactites of the white carbonate of zinc resembling porcelain, and called *china* by the dealers; cubo-octahedral sulphurets of lead containing antimony, arsenic, and silver; highly diaphanous sulphates of barytes; the variety of pearl spar called satin spar; primitive crystals of lime spar, which do not contain magnesia; together with innumerable associations of crystallized quartz, fluate, and carbonate of lime, in every variety of colour and form.

I have the honour to be, dear Sir,

Yours faithfully, E. D. Clarke.

ARTICLE IV.

Observations on Mr. Herapath's Theory.

(To the Editor of the *Annals of Philosophy.*)

SIR, *Oct.* 12, 1821.

HE who has published a theory on any point in philosophy will consider its neglect as the greatest evil. A candid examination of its correctness will tend more to excite attention to it, than almost any degree of praise which can be given. If, then, it be founded in truth, examination and attention are all its author can desire; but if in error, its exposure is all he ought to expect. Mr. Herapath will, therefore, have no right to complain of the following observations upon the theory which he has published "On the Causes, Laws, and principal Phænomena, of Heat, &c."

In the October number of the *Annals of Philosophy*, Mr. Herapath has answered two letters addressed to him upon the subject, by extracts from other letters received by him from some distinguished chemists, in which, while they politely praise the ingenuity of the theory, they express their want of satisfaction as to its truth. I confess I do not see how this can be any answer to, or how indeed it can have any other effect than that of justifying, the doubts of those who have offered particular objections to the theory; for surely it is probable, that if it had been conclusive, such able men as Sir H. Davy and Mr. Gilbert would have perceived its correctness.

Mr. H. has then added that he considers those extracts will be a sufficient justification to him in not replying "to every one who chooses to publish his undemonstrated opinions" on the subject. Of course it cannot be expected that he should answer all objections to his theory; many of them will very probably deserve no notice. The only observation, therefore, that I should make in relation to this determination is on the distinction which he has made as to the objections being *undemonstrated.* If he mean that he does not consider himself bound to regard any objections that are not mathematical demonstrations, I apprehend he has determined to neglect those objections which he ought to fear as the most formidable.

That this was, however, the intention of Mr. Herapath would seem probable from the following sentence: "To avoid metaphysical difficulties, the principles might be passed over by the admission of a simple axiom in philosophy; namely, that it is impossible by correct reasoning from false principles to bring out true conclusions; and hence the attempts at refutation may be confined to the mathematics and the results."—(*Annals* for Oct. p. 307.) It certainly is difficult to deduce from this axiom any meaning which can be supported by argument. In innumerable instances (if the words are taken in their usual sense), true conclusions may be brought out from false principles by correct reasoning. If, for instance, the errors on each side should exactly compensate each other, the result will be correct though the foundation be erroneous. Frequently too there are several ways by which a fact may be accounted for by correct reasonings, yet all those ways cannot be the true mode of accounting for it. The argument in relation to the nature of muriatic acid is a striking instance. The principles, therefore, on which the reasoning is founded in those cases in which the proper mode of explaining the facts is not adopted, are incorrect, though the fact be itself true. Indeed it might be said to have become almost a proverb, that the conclusion may be true though the foundation of the argument is false.

But to return, I apprehend that one of the first objections to the theory which will offer itself to the mind of an inductive philosopher, is its assuming to be so entirely founded on mathematical demonstration. The most important question proposed is, whether heat is a peculiar motion of the particles of bodies. Now it is in the nature of things impossible to demonstrate this to be the fact merely by mathematics. Even if it should be proved, that if the fact be first assumed, the phænomena of heat will be governed by certain laws, and that these laws are the same with those which experiment prove actually to exist, and this be shown to be the case universally, a strong argument would certainly be raised that the phænomena of heat are in fact produced by this peculiar kind of motion; but if any one should therefore assume that he has mathematically demonstrated that heat and the peculiar motion are the same, the assumption will be both illogical and incorrect. One thing is, however, absolutely necessary even to raise an argument in its favour; namely, that the laws discovered by the mathematical reasoning and by experiment, should be indentical *in all cases;*—a circumstance which it must be always most difficult to prove. In this particular case, I will examine whether it is not sufficiently easy to prove the contrary.

Experiment has clearly shown that caloric, or the immediate cause of heat, whatever it may be called, cannot be destroyed. However, under particular circumstances, it may become for a time imperceptible, it can be again developed, and so be shown to have continued its existence; if, therefore, heat and motion be identical, motion cannot be destroyed. This, I apprehend, the experience of every day, in addition to mathematical argument, tells us is untrue. We all every day see motion generated and destroyed. Nor can this objection be answered by a supposed difference in the nature of the motion, as we cannot even conceive of any difference in motions, except that which is made by their quantity and direction.

Again; heat is communicated from one object to another at a distance, without contact, and without materially affecting the temperature of the intermediate air. Motion cannot be so communicated; heat, therefore, cannot be motion. Here indeed it is possible Mr. H. will call in aid the ethereal fluid which he has gratuitously supposed to fill all space. The only proper answer to such a supposition is, 'Show this fluid to me; prove its existence by some other evidence than its being necessary to support your theory; for that argument can have little weight which founds the truth of a theory upon a supposed fluid, the existence of which fluid itself rests only upon the truth of the theory.' But still, grant the fluid to exist; for when once the basis of experiment is departed from, and imagination is let loose, there is no reason why its liberty should be circumscribed. This ethereal fluid, must, however, for the purpose, be supposed capable of receiving and communicating temperature, and this power cannot, by any mode of reasoning, be confined to its relation to bodies distant from each other. But a power of receiving and communicating temperature must most evidently be perceptible to experiment; and we have, therefore, a fluid with evidently perceptible qualities, which is utterly incapable, either by its qualities, or in any other way, of being perceived.

But again, Mr. H. assumes as one of the bases of his theory (of course without any proof), "that what we call heat, arises from an intestine motion of the atoms, or particles, and is proportional to their individual momentum."* These particles he has assumed to be "of different sizes and figures" in different bodies, and the temperature of these bodies he has considered to be equal, when the velocities of their particles are inversely proportional to their magnitude; that is, when the momenta of the particles are equal. The velocities, therefore, of the particles of different bodies will be different at the same temperature.

Now if a body, A, be placed in contact with another body, B, having atoms of a greater magnitude, but a velocity less in an inverse proportion, that is, according to Mr. H. being of the same temperature, "the atoms will be continually impinging on one another, and on the side of the adjoining body." Now it is evident that the atoms of A may impinge upon the atoms of B, whether they be approaching A or receding from it; that is, the atoms of A having a greater velocity, may either meet or overtake the atoms of B; and the probabilities will be nearly equal as to the one or the other. But whether the atoms be elastic, or hard, having the properties of elastic bodies which Mr. H. has attributed to them, or be hard with the properties usually ascribed to hard bodies, still if one atom *a*, of the body A, having a greater velocity than the atom *b*, of the body B, overtake the slower atom, the atom *a* will lose some of its velocity, which will be communicated to the atom *b*, and thence among the other atoms of the body B. The communication of motion from the atoms of A to the atoms of B will not be compensated; for the atoms of B having less velocity than the atoms of A will never overtake them. The motion of the atoms of the whole body B, therefore, will be increased; so that if one body A have atoms of a less magnitude than the atoms of a body B with which it is in contact, but with a velocity inversely greater; that is, according to Mr. H. the bodies A and B being of the same temperature, the momentum of the atoms; that is, the temperature of the body B shall continually increase. But this we know is contrary to the real fact; therefore the temperature of bodies is not the same as the momentum of their atoms moving among each other with different velocities.

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* *Annals of Philosophy* for April.

I could hardly claim room in your pages to trace out all the contradictions to known facts which will be the necessary result of Mr. Herapath's theory: it will be probably more obedient to his wish that I should examine the mathematical demonstration.

Almost the whole of this theory is founded upon Prop. 2 and its corollaries (*Annals of Philosophy*, April, p. 284.) The proposition is, "If a hard spherical body impinge perpendicularly on a hard fixed plane, the body will, after the stroke, remain at rest." This will not be disputed. Mr. H. states in support of it, "that action and reaction are equal." "The force, therefore, with which the ball is acted on by the plane at the time of the contact in *a direction opposite to its motion* is just equal to its momentum; consequently the motion and action destroy one another, and the ball, having no other tendency, continues at rest." This reasoning is I admit indisputable, and being so, it follows that whenever a hard spherical body shall be acted on by a force "in a direction opposite to its motion" "just equal to its momentum," that force and momentum "destroy each other."

Let A be a hard ball having a given momentum; now bodies act with a force equal to their momentum, and so it is assumed of the ball in the foregoing proposition. But if there be another body B similar, and having similar velocity to the body A, the momentum of B shall be equal to the momentum of A; and, consequently, the force with which B acts shall be equal to the momentum of A. If, therefore, B moving in an opposite direction to A meet A, it will act upon it "with a force in a direction opposite to its motion just equal to its momentum," and consequently the momentum of A and the action of B, "being equal and opposite, destroyed each other." This would seem to be necessarily deducible from Mr. Herapath's own proposition and reasoning. But no, says Mr. H. "If two hard and equal balls come in contact with equal and opposite momenta, they will separate with the same velocity with which they met." Let us examine the reasoning. "Suppose a hard plane, or other body, be held against a fixed hard body, and in this way receive the impulse of the ball; then because that part of the intermediate body which is against the fixture is not urged any way by that fixture, the force with which the ball comes in contact with the other side is the force with which the sides of this intermediate body are driven together; but this force is the momentum of the ball; therefore, that momentum is the force of constipation in this case."

But Mr. H. has just before stated, that the plane "re-acts upon the ball at the instant of contact" "in a direction opposite to its motion," with a force "just equal to its momentum;" and consequently the intermediate body would be acted on upon one side by the momentum of the ball, and on the other by the reaction of the plane, which he has stated to be, and which is in fact, equal to the momentum. The force of constipation must necessarily, therefore, be the sum of the forces of the momentum of the ball, and the reaction of the plane.

Mr. H. proceeds: "But if we now fix the intermediate body, and instead of the fixed body on one side of it, imagine another equal ball to come in contact with it at the same time as the former, and with an equal momentum, then the force with which each surface of this intermediate body is urged towards its centre, is equal to the momentum of each of the balls; and, therefore, the force with which the two surfaces are urged together is equal to the sum of these momenta, or to twice one of them; but this force is manifestly the force with which the two balls would have come in contact if there had been no intermediate body; therefore, that force is the double of the force with which either body would have struck a fixed plane." No doubt it is so, and also double the force with which either one ball strikes the other. And as "action and re-action are equal, and contrary," the plane resists the stroke with a force "just equal to its momentum," and the one body resists the other with a force just equal to its momentum. So, if one ball, A, be fixed, and an intermediate body of such a nature as that it shall not be necessary for its *vis inertiæ* to be noticed, be placed in contact with it, if another body, B, with any given momentum, comes in contact with the intermediate body, the two surfaces of the intermediate body will be urged towards its centre with a force exactly as great as if each side had been struck with a momentum equal to that of B. This, besides its being an actual fact, as Mr. Herapath may at any time prove by experiment, necessarily follows from the axiom which he himself has mentioned, that action and re-action are equal; for if that axiom be true, the re-action of the fixed ball must be exactly equal to the action of the ball in motion; and that it is so, is also proved by the fact that the ball requires to be fixed with strength sufficient to afford such a resistance, otherwise it would be driven away. Mr. Herapath, however, from the reasoning in the foregoing extract, immediately concludes; "Hence if two hard and equal balls come in contact with equal and opposite momenta, they will separate after the stroke with the same velocity with which they met. For since the intensity of the stroke *is the force with which each of the balls is acted on in a direction opposite to that in which it came at the time of the contact;* and since that intensity is, by the preceding cor. equal to twice the momentum of either ball, each ball at the time of the contact might be conceived to be acted on by two opposite forces, one its momentum, impelling it towards the other ball; and the other, the force of the contact equal to twice its momentum impelling it in an opposite direction. The difference between these two forces, therefore, or the value of one momentum is the force with which each ball retraces its path; and, consequently, the velocity of the separation of the balls is equal to the velocity of their approach." How Mr. H. proves, "that the intensity of the stroke is the force with which each of the balls is acted on *in a direction opposite to that in which it came* at the time of the contact," I am at a loss to discover; there certainly is nothing suggested in the paper under observation even pretending to be an argument to that effect. The intensity of the force is "equal to the sum of the momenta" "with which both balls come in contact," half of which is in one direction, and half in the opposite; so that the intensity of the force of contact, according to his own previous reasoning, is exactly double to that of each ball in the direction in which it came at the time of contact; consequently, "if each ball at the time of the contact be conceived to be acted on by two opposite forces, one its momentum impelling it towards the other ball, and the other the force" at the time "of the contact impelling it in an opposite direction," which will be half the sum of the momenta; that is, exactly equal to the momentum of one ball, each ball will remain at rest, instead of separating in opposite directions.

Thus if a man push with all his strength against a wall, say with a force as 10, action and re-action being equal, the wall resists with a force as 10, exactly in a similar manner to the fixed plane in Mr. H.'s proposition. If, instead of the wall, there be an opposing active force, another person, for instance, pushing against the first with an exactly equal force, the effect to the first will be just the same as the wall, and neither person will be able to move the other. But by Mr. Herapath's reasoning each person would be acted on in a direction opposite to that towards which he pushed, by a force equal to twice the force of either one; that is, with a force as 20, and consequently both must be pushed backwards; a conclusion notoriously contrary to fact. And yet this is the reasoning by which are to be overturned, in one short page, the doctrines of Newton, Maclaurin, Hutton, Playfair, and innumerable other mathematicians, in relation to the collision of hard bodies; the first principles of which too are as nearly as possible self evident.

I hardly think it is necessary to examine further the mathematical demonstrations of the theory, it being so entirely founded upon the proposition and corollaries which have been the subject of the preceding observations. The next proposition, however, may, perhaps, afford a few remarks.

"If a hard ball strike another hard ball at rest in the line of their centres of gravity, an exchange of state will take place; the former will remain at rest after the stroke, and the latter will proceed in the same direction in which the first was moving, and with the same momentum." (*Annals of Philosophy* for April, p. 287, Prop. 3.)

The following is the reasoning in support of this proposition, rejecting that part of it which is collateral to the argument, and using words instead of algebraic signs, which offer difficulties to persons unaccustomed to them. "If we suppose A (the moving body), so small as to have a ratio to B (the quiescent body), less than any assignable ratio, the ratio of the motion of A after the stroke to the motion of A before the stroke will also be less than any assignable ratio. Therefore the motion of A after the stroke will be unassignably small; *that is, the body A will remain at rest.* And because the motion of A after the stroke is indefinitely small compared to the motion of A before the stroke, the intensity of the impulse will likewise be equal to the momentum of the moving body A before the stroke. But since the intensity of the impulse is the force acting upon the quiescent body at the time of the impulse, it is also equal to the motion acquired by this body. Therefore if a hard ball, &c."

And this is mathematical demonstration! This of course justifies Mr. Herapath in that dignified condescension with which he gracefully and decorously considers that former mathematicians, and Sir Isaac Newton among the number, have been mistaken, not so much from absolute incapacity as from want of attention; and to suggest that had they imagined the consequences deducible from the collision of hard bodies, they would have scrutinized it with greater care. It is wonderful how important is the consequence from so simple an assertion; because "the motion of A is unassignably small, therefore it has no motion at all;" that is, because a thing is unassignably small, it does not exist. Beautiful reasoning! Conclusive argument! Invincible demonstration! Having too infinitely greater force from its wholly relating to things (atoms and their motions) which are all unassignably small, and, therefore, according to Mr. Herapath, which do not exist.

Again: because if A be unassignably small, its motion after the stroke is unassignably small, that is, it has no motion; therefore, "if a hard ball" (having any magnitude whatever) "strike another hard ball in the line of their centres of gravity," its motion after the stroke would be unassignably small; that is, it would have no motion. What can be more unanswerable?

There is, however, one *advantage* in this mode of reasoning which of course I ought not omit to mention. It is well known that any one who publishes a theory is frequently attacked on opposite sides. For instance, while I am endeavouring to show that this proposition is not founded in truth, another person may attempt to prove that if it were, the consequences would be such as directly to contradict the supposition that heat is motion; for it may be said, that if it be true, "that when one hard body strikes another in the line of the centres of gravity, an exchange of state will take place" then if a body the particles of which have any degree of momentum (that is, a heated body), were brought into contact with a body, the particles of which had no motion (that is, an absolutely cold body), an exchange of state would take place; but the effect, which would be absolute in the extreme, would be proportionate in the mean. If one body, therefore, were brought into contact with another whose particles had individually a less degree of momentum, a change of state would take place in proportion to the difference in the momentum of their particles; that is, if one body were brought in contact with another body having a less temperature, an exchange of state would take place. This, however, is contrary to the fact; the surplus temperature would actually be divided by them. But here a distinguished excellence of Mr. H.'s reasoning comes into use; for in order to meet this opposite argument, it is only to change the terms, and the same reasoning may be made to prove exactly the contrary to what it proved before; like the newly-invented steam-vessels, which can sail backwards or forwards with equal ease. Thus let the body, B (the quiescent body), be supposed to have a ratio to A (the moving body), less than any assignable ratio instead of the reverse, and, *mutatis mutandis*, the argument will stand thus: 'If we suppose B so small as to have a ratio to A less than any assignable ratio, the ratio of the loss of the motion of A by the stroke, to the motion of A before the stroke, will also be less than any assignable ratio; the difference in the motion of A, therefore, before and after the stroke, will be unassignably small; that is, they will be just the same!' It will be easily seen that the whole argument may thus be reversed, so that, in a manner most felicitous, the same course of reasoning which proved that an exchange of state between the balls will take place, may be made to prove that such an exchange of state will not take place.

But the proposition is in itself worthy of being repeated. "If a hard ball," (for example, one foot in diameter) "strike another hard ball," (of the magnitude of a pin's head,) "at rest in the line of their centres of gravity, an exchange of state will take place, the former," (the large ball,) "will remain at rest after the stroke, and the latter," (the pin's head ball,) "will proceed in the same direction in which the first was moving, and with the same momentum."

What can be more self evident? It really seems a pity that Mr. Herapath should have expended so much time in demonstrating by the mathematics that which is in itself as self-evident as that two and two make five. I only wonder how the cannon balls with their hard particles can get on, when they strike the hard particles of the atmosphere in the lines of their centres of gravity.

But to draw these observations to a close. Mr. Herapath has expended, I do not doubt, a degree of labour and industry in the formation of his theory, which, well directed, would have done him great honour; and in giving any thing like plausibility to arguments founded on such propositions, he has exhibited very considerable intelligence and ingenuity. But he has in truth quite mistaken the road to philosophical science. He must content himself to travel along the beaten path of the inductive philosophy; it is the only course by which he will make any progress in arriving at truth in natural philosophy. Let him either ascertain new facts by experiment and observation, or reason from facts already known to new and more general laws and principles; and from the patience and intelligence he has already exhibited, there is no doubt but he will both benefit science, and acquire reputation for himself. But if he take another course, and first supposes facts the existence of which he cannot prove, and then endeavours to build upon those mere figments of the imagination, a grand system of nature, he will assuredly ultimately find the foundation give way under him, and gather from it only mortification and regret. True scientific discoveries never have been, and if we can judge from experience, never will be, so made. The Royal Society was originally founded after Lord Bacon's idea, to oppose such a method of reasoning, and establishes one more consistent with sound philosophy. Mr. Herapath, therefore, ought not to wonder that that Society should at first have rejected his paper. On the contrary, the scientific world should rather be surprised that they ultimately admitted into their Transactions a theory founded only on gratuitous assumptions, and on supposed laws of collision of bodies, as contrary to truth, as they are to those principles which have been admitted as incontrovertible by the ablest mathematicians in all ages.

I remain, Sir, yours, &c. C.

Article V.

On the Origin of the Name of Calomel. By Mr. W. R. Whatton.

(To the Editor of the *Annals of Philosophy.*)

SIR, *Manchester, Sept.* 10, 1821.

In the *Annals of Philosophy* for October and December last, are inserted some observations and queries relative to the origin of the name of *calomel,* and the manner in which the mercurial preparation, usually known by that appellation, became so designated.

With a view to the elucidation of this subject, I have consulted the following authors:

Joan. Baptist. Montani, Medicina Universa, pub. 1587.
Hieronimi Mercurialis Opera, died 1606.
Joannis Renodei Dispensatorium, pub. 1615.
Barthol. Perdulcis Universa Medicina, died 1621.
Sennerti Opera, died 1637.
Theodori Turqueti Mayennæ Praxeos in Morbis Internis Syntagma, died 1655.
Ejusdem Turqueti Opera.
Riverii Observationes, died 1656.
Ejusdem Riverii Praxis Medica.
Twelferi Pharmacop. August. Reformata, pub. 1661.
Joan. Baptist. Sitoni Miscel. Med. Curios. pub. 1677.
Raymondj Jo. Fortis Consult. et Respons. Centuriæ Quatuor, pub. 1677.
De Blegny Zodiacus Medico-Gallicus, pub. 1682.
Lexicon Medicum Castellanum, pub. 1682.
Boneti. Index Medico-Practic. pub. 1683.
Nichol. de Chesneau Observationes, pub. 1683.
Jo. Hartmanni Oper. Omn. Med. Chym. pub. 1684.
Freind de Purgantibus, pub. 1719.
Boerhaave de Medicamentorum Viribus, pub. 1720.
Ejusdem Boerhaavij Materia Medica, pub. 1720.
Quincy's Complete Dispensatory, pub. 1720.
London Pharmacopœia, pub. 1720.
Clarkii Hist. Lumbricorum, pub. 1725.
Hoffmanni Clavis Pharmacop. Schroeder, pub. 1742.
Pharmacop. Edinburgensis, pub. 1744.
James's Medical Dictionary, pub. 1745.
Lewis's Experimental Hist. of the Mat. Med. pub. 1768.
Alston's Lectures on the Mat. Med. pub. 1790.

Of these, Montanus, Mercurialis, Renodeus, and Perdulcis, do not mention any other preparation of mercury than the unguentum hydrargyri, and the sublimatum; Sennert is the first who notices the mercurius dulcis, and its method of preparation; and none, prior to Quincy in 1720, speaks of calomel, with the exception of Turquet, De Riviere, and Bonet.

Sir Theodore Turquet de Mayenne, knight, and Baron of Aubonne, was a Frenchman, and born in the year 1572. He took the degree of Bachelor of Medicine at Montpelier in 1596, and the Doctorate in 1597. He was a scholar and chemist of the first eminence, and Physician to the King of France; and in 1616 was invited to England by the British Ambassador, where he successively became first Physician, by patent, to James I. and Charles, and died very rich, and with a high reputation, at Chelsea, in 1655. He wrote Praxeos Mayenniæ in Morbis Internis Syntagma, and the Opera Medica, both which were published after his death, the one in 1690, and the other by Dr. Browne in 1703.

Sir Theodore Turquet is the earliest author to whom I have been able to trace any mention of calomel, and that not as a new preparation, but merely as a name of his own choosing, expressive of the qualities of the mercurius dulcis of Sennert.

As Turquet was a physician in most extensive practice, an excellent and experimental chemist, and a man of high rank in the service of the King, and every where enjoying the greatest popularity, it is not improbable that to him will attach the merit, if any exist, of adopting the curious designation in question. At the end of his last work is given an ample Pharmacopœia, including a large number of chemical preparations of different kinds of his own invention, among which stand the mercurius niger, or æthiops mineralis, and the clyssus mercurii, very similar to the mercurius dulcis, except that it was only three times sublimed, and afterwards well washed in cinnamon or rose water. This form of preparation had its name from the Greek κλυζω, to wash.

In the course of his works, Sir Theodore makes use of the terms pulvis calomelas, ☿ calomelanicus, mercurius calomelanicus, calomelanicus sublimatus dulcis, and calomelanicus optim. præparat. indifferently, by all which he means to express himself as speaking of the submurias hydrargyri. At p. 20, lib. 2, he writes, as if feeling his way in the use of a new formula, "D. Brochant sumpsit mercur. calomelanic. et Guttæ a ℈ss; nauseam levem passus est citra vomitum, dejecit duodecies, et biliosa;" and some time after we have the doses, accompanied by this observation, "mihi notæ et millessima experientiâ fælicissimè comprobatæ mercurii præparationes sunt, aquila rubra, pulvis calomelas, mercurius lunaris, præcipitatum album, et (quod meum est inventum) clyssus metallorum. Horum doses sequuntur: aquila rubra datur per se a gr. xij ad gr. xx. c. theriacâ; calomelanici sublimati dulcis a ℈j ad ʒss; mercurij lunaris gr. vj. and gr. viij, vulgaris dosis gr. vj; clyssj (sive mercurij universalis) ℈j ad ʒss.

Contemporary with Turquet were Du Chesne and De Riviere, and in compliment to the former, we meet with a composition called after his name; Turquet, therefore, tells us, that the "Pilul. *Quercitani* constabat ex ʒj vel ʒss coch. minoris et gr. xij. merc. calomelan." This, moreover, was the celebrated panchymagogus Quercitani; and the mercurius dulcis mixed with scammony, noted by Mr. Gray in his communication of Dec. last, as spoken of by Riviere, is, in like manner, the *Calomelanos Turqueti*, given in his Observationes, and not in the Praxis; and also in the Epistola apud Hildanum of Doringius, noticed by Bonet, and so called after Sir Theodore. To prove that mercurius dulcis and calomel were one and the same preparation, I quote from the Syntagma, p. 287, de Hydrope, a sentence of a case "datum Chelsej, Junij 26, 1651. Elaterium commodè et fæliciter cum merc. dulci jungitur; viz. ejus gr. ss vel ad summam gr. j cum. gr. xij. xv. vel xx merc. dulc. calomelan." Of the origin of the word various solutions have been offered. Quincy thinks the mercurius dulcis was called calomel after the sublimation had been frequently repeated; and so says the London Pharmacopœia for 1720. Gmelin's notion, from μελι, honey, alluded to by Mr. Gray, might do, as in apomel, hydromel, and oxymel, but, unfortunately, we have calo*melanicus* sublimatus *dulcis*, a repetition by no means necessary.

Dr. James gives καλος, good, and μελας, black, from its virtues and colour, and says, that "it formerly meant mercury well pounded with sulphur, and reduced to a black substance, but now calomel, in the common acceptation, means the mercurius dulcis six times sublimed." Dr. James is followed in this idea by Dr. Alston, Dr. Hooper, and many others.

The application of the name, as here specified, is, I think, sufficiently controverted by the fact, before observed, that the mercurius niger, or æthiops, was an invention of Turquet's, and is not mentioned until after the use of the word calomel had become frequent. They are also to be found in the same pages in different prescriptions, and could not be one preparation.

It is true, however, that from the trituration of the oxymuriate with the current mercury, a dark cineritious tint is produced, by which, before the sublimation, the latter part of the name might in some measure be accounted for; but when the complete process for producing the submuriate is gone through, that is, removed, the powder assumes a most beautiful white. Was the word, therefore, indicated by the different appearances of the two stages of preparation? and would the use of καλος and μελας so applied be a sufficient explanation of the term? I should conceive it would not. As a last resource then, what would be thought of the suggestion that the enigma might possibly be solved by καλος, good, or excellent, and μήλης, a searcher, from μηλόω, to search.

I observe Turquet himself never uses the word calo*melanos*, as others do after him, but only calo*melas* and calo*melanicus*; the idea, therefore, will be less objectionable, inasmuch as it will not be necessary to account for the first of these terms, if it originate from an erroneous conception of Turquet's meaning, and one may suppose the second and third to be latinised in the same way, for example, as panchymagogus, from *παν*, all, χυμος, humour, and *αγω*, to draw, a conceit of Du Chesne's, [illegible] now minded.

The synonyms met with in the course of these inquiries are:

Aquila alba,
Aquila cælestis,
Aquila mercurii,
Aquila mitigata,
Calomelas,
Calomelanos,
Calomelanos Turqueti,
Calomelanicus subl. dulc.
Draco mitigatus,
The dragon tamed,
Dulcified mercury,
Dulcified sublimate,
Manna cælestis,
Manna mercurii,
Manna metallorum,
Mercurius dulcis,
Mercurius dulcis sublimatus,
Mercurius dulcis vulgaris,
Mercurius dulc. officinalis,
Mercurius edulcoratus,
Mercurius dulcificatus,
Mercurius sublimat. dulcis,
Mercurius calomelanicus,
☿ calomelanicus,
Mercurius loticus,
Panacea mercurialis,
Panchymagogus Quercitani.

I am, Sir, &c.
W. R. Whatton.

Article VI.

Astronomical Observations, 1821.

By Col. Beaufoy, FRS.

Bushey Heath, near Stanmore.

Latitude 51° 37′ 44·3″ North. Longitude West in time 1′ 20·93″.

Oct. 24.	Emersion of Jupiter's third satellite	10h	34′	22″	Mean Time at Bushey.
		10	35	43	Mean Time at Greenwich.
Oct. 28.	Emersion of Jupiter's first satellite	9	09	51	Mean Time at Bushey.
		9	11	12	Mean Time at Greenwich.
Nov. 4.	Emersion of Jupiter's first satellite	11	05	19	Mean Time at Bushey.
		11	06	40	Mean Time at Greenwich.
Nov. 6.	Emersion of Jupiter's first satellite	5	32	50	Mean Time at Bushey.
		5	34	11	Mean Time at Greenwich.

N. S. Plate XII.

ARTICLE VII.

Description of a new Wind Guage, or Apparatus, for determining the effective Pressure of the Wind upon a given Surface. By Col. Beaufoy, FRS. (With a Plate.)

(To the Editor of the *Annals of Philosophy*.)

DEAR SIR, *Bushey Heath, near Stanmore, Nov.* 18, 1821.

I HAVE the pleasure of sending a drawing and description of a new anemometer, which I find from experience is capable of measuring the momentum of the wind with great accuracy.

Several instruments of this kind have already been constructed with more or less skill; but as the generality give merely the relative impulse, I shall only notice three of the best.

The first was invented by Mons. Bouguer, and is fully described in his learned and scientific "Traité du Navire, de sa Construction et de ses Mouvemens," published in the year 1746. The second was constructed by Dr. Burton, and its machinery is detailed in Mr. Martin's second volume, p. 211, of the Philosophica Britannica. The third is that of Dr. Lind, an account of which is to be found in Dr. Hutton's Mathematical Dictionary. M. Bouguer's instrument is exceedingly well adapted for making experiments at sea; but I found it could be rendered more accurate by cutting teeth in the slider to turn wheel-work, to which hands were attached similar to those in the accompanying plate. The anemometer of Dr. Burton shows only the relative impulse; but the absolute force of the wind may be determined by referring to p. 94, vol. viii. of the *Annals.* The disadvantage of Dr. Lind's wind guage is, that in stormy weather, the water is liable to be blown out of the tube. It was with the machine I am now about to describe, that some of the experiments were made recorded in p. 277, vol. vi. of the *Annals.*

I remain, dear Sir, truly yours,
MARK BEAUFOY.

THE apparatus consists of a thin board or screen, one foot square, having a brass bar projecting perpendicularly from its back surface in the middle thereof; this bar is supported and guided between eight brass rollers, arranged in a fixed frame in such manner that the bar and screen may be capable of sliding freely backwards and forwards, by the action of the wind upon the surface of the screen, when the apparatus is placed with the bar in a horizontal position. The force with which the wind acts upon the screen is ascertained by a weight suspended by a silken cord passing round a spiral or fusee; upon the axis of which, a small cylindrical barrel is fixed having a chain (similar to those used in clocks) winding upon it; the other end of the chain is attached to the sliding bar, so that when the screen is pressed backwards, it draws the chain, and turns the barrel and fusee round, drawing up the weight, which acts with an increasing power as the screen is forced further backwards, by the silken cord winding upon a larger radius of the fusee. The axis of the fusee is furnished with a ratchet wheel (and pall) to retain it, and prevent any retrograde motion; in order that the index upon the axis of the fusee may indicate the extreme point at which the screen had been forced by the wind at any period of the experiment.

The construction of the apparatus is particularly described in Plate XII. Fig. 1 represents a perspective view of the whole apparatus, placed in a situation to act; it is mounted upon a mahogany tripod stand, similar to the portable instruments used in surveying. The upper part of the apparatus is surmounted by a light stick bearing a small silk flag upon its top, to show the direction or quarter of the wind, in order to place the surface of the screen perpendicular to the direction of the wind's motion, previous to making an experiment upon the force of it.

Fig. 2, represents a side elevation of the apparatus with only part of the sliding bar, B B, shown.

Fig. 3, a plan of the whole apparatus.

Fig. 4, a side elevation with some parts of the frame removed to explain the internal works; and figures 5, 6, and 7, represent transverse sections of the apparatus taken at different parts of its length. The same letters of reference serve to denote similar parts upon each. A A, the screen which is attached by a screw pin *p* to the extreme end of the bar B, B. *a a*, *b b*, represent the four rollers which serve to guide the bar horizontally; and *c c*, *d d*, those which guide it sideways, as seen in fig. 3. C and D, the chain, one end of which is fixed to the underside of the bar at C, and the other end winds round the barrel D upon the axis of the fusee F. G shows a silken cord, which winds round the fusee, having a weight H hooked to the lower end of it; E, the ratchet wheel upon the fusee axis, furnished with a click or pall, I, to prevent the descent of the weight, after it has been drawn up to any particular point by the action of the wind upon the screen A. K K, represent two brass plates, which receive and support the axis of the fusee, as also the axis of the pall I, and the pivots of the two rollers, *a a*. The plates, K K, are kept parallel by three small pillars, S S S (in the manner of clock movements), and are attached by screw bolts, *e e*, to the wooden block M, which forms the principal frame of the apparatus. The wood rises up at N, and is hollowed out to contain the small rollers, *b b*; the pivots of which turn in brass plates, *h h*, screwed on each side of the block, as seen by the dotted lines in the plan, fig. 3.

The vertical rollers, *d d*, are supported by brass cocks, *i i*, screwed against the plates, *h h*; the pivots of the other vertical rollers, *c c*, also turn in cocks, *k k*, screwed to the plates K K, as seen in the transverse section, fig. 5. O, shows a circular brass plate, fixed upon the upper part of the tripod stand; and P another plate connected with the block, M, by a brass fork, Q, and centre pin, R, as may be distinctly seen in fig. 7, forming a turning joint capable of a slight motion to adjust the apparatus into an horizontal position, which is determined by the spirit level, L, fixed upon the block, M. The motion upon the centre pin, R, is regulated by two milled head screws, T T, passing through a strong brass cock or angle piece, V; fixed by three screws to one side of the fork, Q, the ends of the screws, T, bearing upon the top of the block, as may be seen in the section, fig. 7. *m m*, represent two thumb screws, which enter into holes in the plate O, and pass through oblong grooves, *n n*, in the plate P (see fig. 3), to allow of turning the apparatus round a small quantity to adjust it into the direction of the wind. W, shows the dial plate, which is divided into 100 equal parts, and numbered at every tenth division. The dial plate is placed concentric with the fusee axis, which has a needle, *w*, fixed upon the end of it, to point out the portions of a turn of the fusee; and the small circle of five divisions upon the dial is furnished with a needle, *x*, moved by a wheel and pinion (situated behind the dial, as will appear in figures 3 and 6), of such number of teeth as to cause the small needle, *x*, to advance only one division during an entire revolution of the needle, *w*. The small needle will indicate the number of turns which the silk cord has made upon the fusee, F. X represents the stick which carries the wind flag upon the top of it.

The apparatus is furnished with weights of various sizes, to be used in strong or light winds; they are adapted to pack up in a case along with the apparatus in order to be removed from one place to another in a convenient manner. The screen, A, may be taken off, and carried separate by withdrawing the screw pin, *p*; Z, fig. 2, shows a small brass hook, which is adapted to enter into a hole formed in the bar, B, to prevent its running out and breaking the chain in setting up the apparatus for use.

To find the value of each division on the dial plate, or power, requisite to move the hand, *w*, and raise the weight, H; one end of a silken line was fastened in the hole, B, fig. 4. To the other extremity was hung a light tin cup, the intermediate part of the cord passing over a well made pulley, suspended from the ceiling of a room; and in this cup were placed a sufficient quantity of small leaden shot, to move the index one division. The line was afterwards detached, and weighed with the cup and shot; the result written down in a table, and afterwards reduced to pounds and decimal parts of pounds avoirdupoise. But as the cylinder and fusee were accurately turned, and the spiral truly cut, it was found not necessary to examine each division, every tenth one being sufficient, with the intermediate blanks filled up by taking proportional parts of the different weights.

Article VIII.

Tables of Temperature, and a Mathematical Development of the Causes and Laws of the Phænomena which have been adduced in Support of the Hypotheses of "Calorific Capacity, Latent Heat," &c. By John Herapath, Esq.

(*Continued from* p. 388.)

Theory of Evaporation.

Prop. XV. Theor. XIII.

If T represent the true temperature of water inclosed in a vacuum, I say the tension of its vapour will be equal to $30 \times (\cdot 002783313\ T - 2\cdot 263714)^{6\cdot 2}$ very nearly, estimated by the pressure of a column of mercury in inches.

I shall not here enter into the investigation of this theorem, because it requires the previous solution of other problems on which I have not yet touched, but shall proceed to give philosophers a few specimens of its accordance with phænomena from the experiments of Mr. Robison, Mr. Southern, Mr. Dalton, and Dr. Ure.

Scholium.

It is necessary to observe, that the constant quantities were determined from Dr. Ure's observations published in the Philosophical Transactions for 1818. These observations are the latest, and in the higher temperatures the most correct I believe that have yet appeared. In the lower temperatures, that is, beneath 212° of Fahr. they very nearly coincide with Mr. Dalton's numbers; a circumstance by no means discreditable to the care and skill of both, and not uncalculated to give us confidence in the rest of the Doctor's results. Unfortunately for myself I have neither Dr. Ure's nor Mr. Dalton's papers at hand, and cannot, therefore, avail myself of their labours to push this part of my inquiries, as it respects other vapours, to the length I could wish; but if ever I should write on the subject of vapours again, I hope I shall have more time before me, and be better prepared to do it justice. At present my object in giving this theorem is to prepare the way for the solutions of one or two principal problems relative to the theory of the steam engine. Philosophers will, in the sequel, see whether I have succeeded or not.

Example 1.—Let the temperature be 32° Fahr. then by Table 3, $T = 1000$, and $30 \times (\cdot 002783313\ T - 2\cdot 263714)^{6\cdot 2} = 30 \times (\cdot 519599)^{6\cdot 2}$.

·519599 log. 9·7156683......A

A × 8 = 7·7253464
A × ·2 = 9·9431337
30 log. .. 1·4771213

·140 log. 9·1456014

The tension of aqueous vapour, therefore, at 32° Fahr. is ·140 in. by Dr. Ure, by Mr. Dalton it is ·20, and by Mr. Southern 16.

Example 2.—Let the temperature be 165° Fahr. then by Table 3, T = 1130·1.

1130·1 log. 3·0531169

·002783313 log. 7·4445621
3·14541 0·4976790
2·26371

0·88170 log. 9·9453208....A

A × 8 = 9·5625664
A × ·2 = 9·9890642
30 log. 1·4771213

10·684 log. 1·0287519

Hence the tension should be 10·684 in. By Mr. Dalton it is 10·68, and by Dr. Ure 10·80.

These are as unfavourable cases for the theory as any I think which can be picked out in temperatures beneath the boiling of water; let us, therefore, see what will be the result in some of the superior temperatures.

Example 3.—Suppose the tension be required at 250° Fahr. as I am not aware that Dr. Ure has made any allowance for the difference between the indications of an air and a mercurial thermometer, I have in computing the values of the constants reduced his temperatures, supposing them mercurial, to air indications. This will make a difference in the values of the constants, which, if Dr. Ure* has accounted for, it will be needful to rectify. Making this re-allowance would not, however, affect the theoretical accuracy of the formula, which would only require its constants to be a little changed to accommodate its results to the new temperatures.

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* I perceive the Doctor has not accounted for it; he appears to consider the mercurial as the true indications of temperature.

By Table 4th 250° require a correction of − ·9°.

$$\begin{array}{lrrr}
\text{Therefore} & 250{\cdot}0^{\circ} & & \\
 & -{\cdot}9 & & \\
\hline
 & 249{\cdot}1 & \text{which gives Tab. 3d } T = 1205{\cdot}1 & \text{log. } 3{\cdot}0810231 \\
 & & a^{*} = & 7{\cdot}4445621 \\
\hline
 & & 3{\cdot}3542 & 0{\cdot}5255852 \\
 & & 2{\cdot}2637 & \\
\hline
 & & 1{\cdot}0905 \text{ log.} & 0{\cdot}0376257 \,..\, A \\
\hline
 & & A \times 8 = & 0{\cdot}3010056 \\
 & & A \times {\cdot}2 = & {\cdot}0075251 \\
 & & b = & 1{\cdot}4771213 \\
\hline
 & & 61{\cdot}05 & 1{\cdot}7856520
\end{array}$$

The theoretical tension is, therefore, 61·05 in. By Dr. Ure's experiments, it is 61·90, by Mr. Southern's 60·00, and by Dr. Ure's formula, it is about 62·95.

Example 4.—It is required to calculate the tension at 295° Fahr. mercurial.

$$\begin{array}{lrrr}
 & 295^{\circ} & & \\
\text{Cor. Tab. 4,} & -2{\cdot}1 & & \\
\hline
 & 292{\cdot}9, & \text{hence Tab. 3, } T = 1242{\cdot}4 & \text{log. } 3{\cdot}0942614 \\
 & & a = & 7{\cdot}4445621 \\
\hline
 & & 3{\cdot}4580 & 0{\cdot}5388235 \\
 & & 2{\cdot}2637 & \\
\hline
 & & 1{\cdot}1943 & {\cdot}0771134 \,..\, A \\
\hline
 & & A \times 8 = & {\cdot}6169072 \\
 & & A \times {\cdot}2 = & {\cdot}0154227 \\
 & & b = & 1{\cdot}4771213 \\
\hline
 & & 128{\cdot}66 & 2{\cdot}1094512
\end{array}$$

The tension by theory is, therefore, 128·66. By Dr. Ure's experiments, it is 129.

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[*] To avoid repetitions, I shall henceforward designate the logarithms of the constant quantities ·00278313 and 30 by *a* and *b*.

Example 5.—Let the temperature be 312° Fahr.

$$\begin{array}{rlr}
 & 312^\circ & \\
\text{Cor.} & -2{\cdot}6 & \\
\hline
 & 309{\cdot}4, \text{ hence } T = 1256{\cdot}2 \text{ log.} & 3{\cdot}0990588 \\
 & a = & 7{\cdot}4445621 \\
\hline
 & 3{\cdot}4964 & 0{\cdot}5436209 \\
 & 2{\cdot}2637 & \\
\hline
 & 1{\cdot}2327 \text{ log.} & 0{\cdot}0908754 \;..A \\
\hline
 & A \times 8 = & {\cdot}7270032 \\
 & A \times {\cdot}2 = & {\cdot}0181751 \\
 & b = & 1{\cdot}4771213 \\
\hline
 & 166{\cdot}84 & 2{\cdot}2222996
\end{array}$$

Dr. Ure's experiments give this tension 167·00 in. and his theorem about 161·05 in. that is, nearly six inches beneath his observation.

Example 6.—Suppose the temperature be 343·6° Fahr.

$$\begin{array}{rlr}
 & 343{\cdot}6^\circ & \\
\text{Cor.} & -3{\cdot}6 & \\
\hline
 & 340{\cdot}0, \text{ which gives } T = 1281{\cdot}2 \text{ log.} & 3{\cdot}1076169 \\
 & a = & 7{\cdot}4445261 \\
\hline
 & 3{\cdot}5656 & 0{\cdot}5521430 \\
 & 2{\cdot}2637 & \\
\hline
 & 1{\cdot}3019 & 0{\cdot}1145776 \;..A \\
\hline
 & A \times 8 = & {\cdot}9166208 \\
 & A \times {\cdot}2 = & {\cdot}0229155 \\
 & b = & 1{\cdot}4771213 \\
\hline
 & 261{\cdot}01 & 2{\cdot}4166576
\end{array}$$

This tension comes out 261 in. By Mr. Southern it is only 240. But if we observe Mr. Southern's tensions in the higher ranges fall much below Dr. Ure's, and that in an increasing ratio. When Mr. Southern makes the tension 60, Dr. Ure finds it 61·9; and when Mr. Southern's is doubled, or 120, Dr. Ure's is above 126; so that in doubling the tension, the difference is more than trebled. If we allow the same proportion of error to hold good in another doubling of the tension, the difference ought to be about 19, which, added to Mr. Southern's 240, would give for Dr. Ure's 259; that is, only 2 less than it ought to be by our theory. Whether such a law between the difference and force of tensions is correct, it would be speculative to affirm; but this seems very evident from a comparison of the experiments, that had Dr. Ure carried his experiments to the same temperature, he would have brought out a much greater tension than Mr. Southern has. It is, however, very curious that Dr. Ure's theorem, according to Dr. Thomson, makes the tension at 343·6° Fahr. only 210, or full 30 in. lower than Mr. Southern's experiment. But if it is so much below Mr. Southern's observations, what would it be below his own if carried to the same length? There is every reason to believe the error would have been at least 50 inch. or upwards of a fifth part of the whole tension.

What could be the cause of Mr. Southern's tensions being progressively lower than Dr. Ure's, it is difficult to say decidedly; but I rather think it must be owing to his thermometer. If the instrument indicated too high a temperature in the higher regions, the errors would undoubtedly commence from the boiling of water, and increase probably proportionally as the temperature ascended. Suppose the error at 250° Fahr. was 1·83°, at 2·934° it would be 3·9°, and at 343·6° about 6·3°. Reducing Mr. Southern's temperatures by these quantities, they become 248·17°, 289·5°, and 337·3°, the temperatures at which the tensions ought to be 60 in. 120 in. and 240 in. By Dr. Ure's observations, it would seem that at 248·17° the tension is about 60·03 in.; and at 289·9°, which is only ·4° higher than 289·5°, it is also about 120 in. So also by the theorem which I have shown to agree so nearly with Dr. Ure's observations the tension at 337·3° is very nearly 240 in. Hence with this simple correction the two observations may be very nearly reconciled.

The examples in the following table I have endeavoured to collect as the most disadvantageous to the theory. In other instances I think it will in general be found that the differences are less. To these examples I have added some calculations from a theorem given by Dr. Ure.

Temp. Fahr.	Tensions in inches of mercury observed by			Calculations from the theorems of		Errors of	
	Robison.	Dalton.	Ure.	Ure.	Herapath.	Ure.	Herapath.
32·0	0·16	0·20	0·20		0·14		—·06
80·0	0·82	1·00	1·01	1·06	0·939	+ ·05	—·071
120·0	3·00	3·33	3·30	3·37	3·300	+ ·07	·000
165·0		10·68	10·80		10·684		—·196
212·0	30·0	30·00	30·00	30·00	30·000	·00	·000
240·0	54·9		51·70	52·46	50·863	+ ·76	—·837
250·0	66·8		61·90	62·95	61·05	+1·05	—·65
260·0	80·3		72·30	74·91	72·49	+2·61	+·19
270·0	94·1		86·30	88·39	85·79	+2·09	—·51
275·0			93·48		93·83		+·35
280·0	105·9		101·90	103·41	101·17	+1·51	—·73
283·8			107·70		107·49		—·21

TABLE *continued.*

Temp. Fahr.	Tensions in inches of mercury observed by			Calculations from the theorems of		Errors of	
	Robison.	Dalton.	Ure.	Ure.	Herapath.	Ure.	Herapath.
285·2			112·20		110·08		−2·12
287·2			114·80		113·52		−1·28
290·0			120·15	119·95	118·93	−0·25	−1·22
292·3			123·10		123·03		−0·07
295·0			129·00		128·66		−0·34
300·0			139·70	137·94	138·80	−1·76	−0·90
310·0			161·30	157·25	161·64	−4·05	+0·34
312·0			167·00	161·05	166·84	−5·95	−0·16

I need make no comment on the very striking, I might almost say perfect agreement between the numbers observed and computed from the theorem I have given. While Dr. Ure's theorem is often 2½ inches, and towards the end as much as 4 and 6 inches in error, in no one instance does mine deviate 2¼ inches; and were the observations perfectly correct, I think it would never err at the most more than about an inch. For about the temperature 285°, when the error is the greatest, the experiments disagree with one another so much that little dependence can be placed on them. Thus the difference between the tension at 285·2° and that of 287·2°, two degrees above it, amounts to only 2·6 inches; while the difference for 1·4° beneath it is equal to 4·5 inches. Now 1·4° : 2° :: 4·5 inches : 6·6 inches. Therefore, taking the difference between the two lower temperatures as correct, and following only the ordinary rate of common proportion, the difference between the tensions of 285·2° and 287·2° should be 6·6 inches; and we know, according to the laws of the increase of tension, the difference should exceed this if the first difference be correct, yet Dr. Ure's experiments give only 2·6 inches. Might we not consequently venture to say that the greater part of the deviation, 2·12 inches, at the temperature 285·2° does not belong to the theorem but to the observation? However, I frankly confess, that for want of having the whole of Dr. Ure's observations when I investigated this theorem, there is a possibility, or, perhaps, a probability, of my not having determined the arbitrary constants to a very great accuracy; and, therefore, it is possible the theorem might not be perfectly consistent with truth in every part of the table; yet I think it may be safely depended on throughout the whole of Dr. Ure's range of observation to an inch, or at most an inch and a quarter. Even to a tension of 600 inches, or 20 atmospheres, I think its error would scarcely exceed 5 or 6 inches. In the higher ranges, where Dr. Ure's theorem begins to diverge from experiment very rapidly, the agreement may be said to amount to a coincidence. For a few degrees above the boiling point Dr.

Ure's theorem will have the advantage; because this is made the point of departure in his theorem, whereas in mine it was only used as one of the conditions. Indeed it is questionable, as I shall presently show, whether philosophers are not deceived respecting the actual tension of vapour at 212°. It has commonly been conceived that the tension of vapour at 212°, and the atmospheric pressure under which water would boil at the same temperature are perfectly equal; but from the views to which I allude, it seems evident that this, strictly speaking, is not the case. The tension, there is no doubt, is at the same temperature greater than the atmospheric pressure of ebullition; but whether at 212° this difference is appreciable is a question I am not experimentally prepared to prove. Let it be tried at a low pressure, or, for instance, at a few degrees of temperature above ebullition in vacuo, and I feel persuaded the difference will be easily detected.

Mathematicians can hardly conceive what labour and trouble the investigation of apparently so simple a theorem has cost me. The time indeed which this theorem has consumed out of the small portion, not more than a month, I have had to discover, to investigate, to make my researches, and to write the present theory of evaporation, combined with other avocations, disadvantages, and an indifferent state of health, has contributed to render the theory and inquiries much less perfect than I could wish. However, the scientific world will not, I presume, be displeased at my having sacrificed a part that can easily be retrieved for the discovery of a theorem which, with others, I shall in the course of this paper develop, will enable us to give the last stroke of perfection to the theory of one of the most powerful and useful instruments of modern invention, the steam engine.

By this theorem, we find the temperature of no evaporation is about 813·3 true temperature, and about 131° of Fahrenheit below zero. Dr. Ure's theorem would put the temperature of no evaporation at an infinite distance below 32° Fahr. and again beyond 450° Fahr. it would make the tension decrease instead of increase with an augmentation of temperature—a conclusion decidedly wrong.

If τ denote the tension, then the true temperature $T = \dfrac{\sqrt[8\cdot2]{\dfrac{\tau}{30}} + 2\cdot263714}{\cdot002783313}$; so that the tension being given, we can easily find the true temperature corresponding; by Dr. Ure's theorem, this cannot be done in a direct and general manner.

This theorem, and those I have already demonstrated, will easily enable us to resolve almost every question relative to evaporation, and the specific gravity, elasticity, &c. of steam; but the application is so easy I forbear entering into it.

I have already pointed out the difference between evaporation and ebullition; and have shown that the one arises from a decomposition at the surface, and is, therefore, influenced by temperature only, and the other from an internal decomposition which subjects it to the influence of pressure as well as temperature. From even a first view of phænomena at this point, one would infer that the temperatures of tension and ebullition, or the temperature of a fluid at the proper tension of its vapour, and the temperature of its ebullition under an equal pressure, are different; the latter being higher than the former. Notwithstanding the obviousness of this difference, philosophers have never, to the best of my knowledge, discovered it; but have confounded the two phænomena together. To set the difference in a clear light, let us descend to the temperature of ebullition in vacuo. By Mr. Robison's experiments, it would seem that all fluids boil at about 124° of Fahrenheit less in vacuo than under an atmosphere of 30 inches. Water, therefore, will boil at about 88° Fahr. in a vacuum. Now, by Mr. Dalton's experiments, the tension at 88° is 1·28 inches; but because an increase of pressure retards the temperature of ebullition, if we put on the surface of the water an atmospheric pressure of 1¼ inch, we must evidently increase the temperature to make it boil; and as it is about the incipient state of ebullition we must probably increase it many degrees. Therefore the temperature at which water boils under a pressure of 1·28 in. is higher than the temperature corresponding to an equal tension. Let us merely, for the sake of continuing the argument, conceive, that with a pressure of 1·28, water boils at 100° Fahr. At this temperature the tension is 1·86, or ·38 above what it was before. Consequently, if we increase the pressure on the water by ·38 so as to make it 1·86, we shall increase its temperature of ebullition, and make it higher than 100°; but at the tension 1·86, the temperature is 100°; hence, therefore, the temperature of ebullition is still above the temperature of tension. By carrying on the argument in this way, it will appear that the temperature of ebullition is always higher than the temperature of tension; though it is manifest that as the tensions increase, the two temperatures will continually approximate, until at very high tensions no sensible difference can be discovered.

The following table, computed from the experiments of Mr Dalton and De Luc, strengthens this inference. De Luc, I believe, deduced his boiling point from a pressure of 29·8 in. and Mr. Dalton from a pressure of 30 inches. The one temperature, 212° of ebullition, corresponds to about 211·66 of the other; I have, therefore, increased Mr. Dalton's temperatures by ·34°, ·34°, ·33°, ·33°, ·33°, ·31°, ·31°, respectively, to make them correspond to a like pressure of ebullition.

Places of observation.	Height of the barometer, English inches.	Temperature of ebullition of water in Fahr. by De Luc.	Temp. in Fahr. by Dalton, the tensions being the same as De Luc's barometric pressures.	Excess of temp. Ebullition above temp. Temper. tension in Fahr.
Beaucaire......	30·105	212·83°	212·50°	+0·33°
Geneva.......	28·835	210·49	210·34	0·15
Grange Town ..	26·122	205·50	205·33	0·17
Lans le Bourg ..	25·732	205·65	204·56	1·09
Grange le F....	25·673	204·71	204·45	0·26
Grenairon......	21·770	196·84	196·51	0·33
Glaciere de B...	20·971	195·26	194·75	0·51

If any corroboration of the simple conclusion I have drawn is wanting, the numbers of the present table, if correct, afford it. Philosophers, however, have been so accustomed to confound the temperature of tension with that of ebullition, that it is become habitual to consider them as one. Into this error it seems Dr. Wollaston has fallen. This gentleman has presented two papers to the Royal Society, describing an instrument which he terms a barometrical thermometer, for measuring the heights of mountains by the temperature of the ebullition of water under different compressions. I have not myself read the Doctor's papers, but by the accounts I have met with, he makes the temperatures of ebullition and tension the same, and computes them from a theorem given by Dr. Ure. However useful and correct Dr. Ure's theorem may be for the determination of the tension in the neighbourhood of 212°, it cannot, from what I have just shown, take from Dr. Wollaston's method the disadvantage of confounding two distinct things, and, therefore, of being itself founded on erroneous principles. I shall make no observation on the probable increase by such a method of the common barometrical errors; nor shall I enter into any discussion of the other merits of the instrument. My object is merely to show that whatever merit may result from any ingenuity of idea, the principles of the instrument are not such as can be called correct, which I have no doubt the Doctor will, on a slight perusal of what I have written, immediately perceive.

At some future period, I may describe a simple portable altimeter I have contrived, that Mr. Trimmer and myself have some thoughts of making, which appears to me to possess all the advantages of a barometer, and to have an almost indefinite precision, with a convenient portability; but now I am anxious to say something of other matters.

Mathematical Laws of the Phænomena of Corpuscular Aggregation and Decomposition, or of the Phænomena adduced in Support of the Hypothesis of Latent Heat, &c.

Prop. XVI. Prob. III.

It is required to determine from experiment and the principles already delivered, the ratio of the baromerins of a given body in the solid and fluid state.

The bodies of which I now intend to treat are those which change their state at a fixed temperature; and as I have not time before me to enter minutely into the peculiarities of each one of this class, I shall confine myself to the consideration of the phænomena of water, which will serve for an example of the way in which like inquiries with other bodies are to be conducted.

Ice below 1000 true temperature brought into a room, or into an air, of a much higher temperature, will gradually become warmer, until it has attained the temperature of its liquefaction 1000. No sooner has its temperature ascended to this point than it continues stationary until the whole ice is melted, however much higher the temperature of the surrounding air may be. But when all the ice is once melted, the temperature will again, progressively ascend to within a trifle of the temperature of the air. During the time the liquefaction is proceeding, the constant communication of temperature goes, as I have already shown, to supply the defects in the individual temperatures occasioned by the decompositions. Thus then distributing a certain additional quantity of motion among the particles of a given quantity of ice may raise it from a certain temperature to the point of liquefaction without melting any of it. Increasing the quantity of that distributed excess will only tend to melt a part of the ice, but have no effect on the temperature, provided it be not more than sufficient to melt the whole of the ice. Hence as it is immaterial in what way the addition of temperature comes, we may conceive it to be communicated from water of a higher temperature mixed with the ice. It is, therefore, evidently possible to find two such quantities of water at a given temperature above 1000, or to find two such temperatures above 1000 for a given quantity of water, that if the two quantities be mixed with two given but equal portions of ice at a given temperature, in one mixture the temperature shall be just 1000 without any of the ice being melted, and in the other the temperature shall be the same, and all of it melted. In the former instance, the baromerin of the ice remains unchanged; in the latter, it is equal to that of water. And the same change in the baromerin of the ice and no more would take place, if the whole of it, instead of being just melted, had been raised to a considerably higher temperature. From these premises we have to determine the ratio of the baromerins.

Call B the baromerin of a given portion of ice, W the weight of it, which we suppose proportional to the quantity of its matter, and T its true temperature. Let b, w, t, denote the same things respectively of a given portion of water, and call τ the true temperature of the mixture cleared from all adventitious circumstances. Then $W B T + w t b$ is the sum of the motions or temperatures of all the parts of the bodies before the mixture, and $(W B + w b) \tau$ would be the same thing after the mixture on the supposition that each body retained its respective baromerin unchanged. Therefore no motion being gained or lost by the mixture, we should have $W B T + w b t = W B \tau + w b \tau$, and

$$\tau = \frac{W B T + w b t}{W B + w b}\text{; the same as we have in Cor. 4, Prop. IV.}$$

However, because if the ice by the mixture be not melted, the water is most probably converted into ice; or if the water be not converted into ice, the ice is into water, the baromerin of one of the quantities must be changed, and either $(W B + w b) \tau$ must become $(W B + w B) \tau$, supposing the water frozen, or $(W b + w b) \tau$, supposing the ice melted. In either case the sum of the motions, as we imagine nothing gained or lost in the mixture, will be the same before and after; so that we have $W B T + w b t = (W + w) B \tau$ when the water is frozen, or $= (W + w) b \tau$ when the ice is melted. In the former case, $B : b :: w t : W T - W \tau - w \tau$, and in the latter $B : b :: W \tau + w \tau - w t : W T$. Hence by knowing W, w, T, t, and τ, we get the ratio of B to b. Q. E. I.

Cor. 1.—Dr. Black, with whom the present doctrine of "Latent Heat" originated, found, by mixing water at 172° Fahr. with an equal weight of ice at 32°, that the whole of the ice was melted, but no addition made to its temperature. According to this experiment $T = \tau = 1000$, $t = 1136{\cdot}5$, and W, w, may each be put equal to 1. Therefore $B : b :: 2000 - 1136{\cdot}5 : 1000 :: 863{\cdot}5 : 1000 :: 19 : 22$ very nearly. Consequently the baromerin of ice is to that of water as 19 to 22 very nearly; and the mass of a particle of ice is to that of a particle of water as 22 to 19.

Notwithstanding philosophers have written so much on the doctrine of "Latent Heat," they appear to be so poor in experiments that I have met with but one besides that from which I have computed the preceding numbers with which I can compare the theorems I have investigated; and that one is to be found in almost every book on the subject. Dr. Black mixed 143 half drachms of water at 190° Fahr. with 119 half drachms of ice at 32° Fahr. In a few minutes all the ice was melted, and the temperature of the mixture found to be 53° Fahr. By Table 3, 190° Fahr. gives 1153, and 32° gives 1000. Therefore

$$\tau = \frac{W B T + w b t}{(W + w) \, . \, b} = \frac{119 \times 19 \times 1000 + 143 \times 22 \times 1153}{(119 + 143) \, . \, 22} = 1021{\cdot}2,$$

which gives 52·6° Fahr. or only ·4° below what Dr. Black determined it to be. So far, therefore, the theory again coincides with all the experiments I have to produce.

Cor. 2.—By the preceding cor. it appears, substituting for B and b their values, that $\tau = \frac{19\,W\,T + 22\,w\,t}{(W + w)\,22}$ when all the ice is melted, and $\tau = \frac{19\,W\,T + 22\,w\,t}{(W + w)\,19}$ when all the water is frozen; W, T, denoting the weight and true temperature of the ice or snow, and w, t, the same things of the water, τ being the true temperature of the mixture.

Cor. 3.—Hence we have $w = \frac{(22\,\tau - 19\,T)\,W}{(t - \tau)\,22}$, all the ice being melted, and $w = \frac{(\tau - T)\,19\,W}{22\,t - 19\,\tau}$, all the water being frozen. From these theorems two curious cases arise, the circumstances of which we can easily determine; namely, that at a given temperature two unequal portions of water may be found, which, mixed with equal weights of snow at a common temperature less than 32° Fahr. or two unequal temperatures may be found at which two equal portions of water in like manner mixed with equal weights of snow at a common temperature less than 32° Fahr. will produce a mixture of the same temperature; that is, 32° Fahr. I can only stop to compute the first of these cases. Let us put $W = 1$, $T = 990$ ($22\frac{1}{2}°$ Fahr.), $t = 1010$ ($41\frac{3}{5}°$ Fahr.) and $\tau = 1000$; then by the first formula $w = \frac{3190}{220} = \frac{29}{2} = 14\frac{1}{2}$, and by the second $w = \frac{190}{3220} =$ about $\frac{1}{17}$. Therefore, if a given quantity of ice or snow at $22\frac{1}{2}°$ Fahr. be mixed with $\frac{1}{17}$ of its weight of water at $41\frac{3}{5}°$ Fahr. the temperature of the mixture will be the same as if the ice or snow had been mixed with $14\frac{1}{2}$ times its weight of water at the said $41\frac{3}{5}°$ Fahr.; that is, in other words, if 17 oz. of snow at 22·5° Fahr. be mixed with only 1 oz. of water at 41·6° Fahr. any additional quantity of water at the same 41·6° Fahr. not above 245 oz. will only melt the ice partially, but not raise its temperature.

Scholium.

The numbers 22 and 19, which we have determined for the baromerins of water and ice, denote also the respective powers of these bodies to affect the temperature of any other body, and, therefore, denote numbers proportional to what are called the "capacities" of these bodies. Now the "capacity of water being 1," Kirwan determined the "capacity of ice to be ·9," and Irvin ·8. The mean of these two is ·85. By the ratio of 22 to 19, it ought to be ·86. Thus then our theory furnishes us with a simple formula, which, from the numbers of a single experiment, enables us to demonstrate the laws and the phænomena which have engendered the hypothesis of "Latent Heat and Calorific Capacity." Will any theorem derived from the hypothesis of caloric enable us to do this? or rather will that doctrine furnish us, from its own principles, with any general and comprehensive formula of the kind? If it will, some one, perhaps, will have the goodness to let the world see it; and to let us know something more of the merits of a doctrine, which I own appears to me to be more entitled to attention from the ability of some of its friends, than for any claims to respect which it has for its innate worth, or for the good it has done towards the extension of science.

Prop. XVII. Prob. IV.

The temperature of a given weight of ice, and also of a given weight of water, being known, it is required to find the quantities of water which shall be in the solid and fluid state after the mixture.

If the water be not enough in quantity or temperature to raise the mixture to 1000, the whole of the water, by the preceding Prop. and Corollaries, will be frozen; and if it be enough to raise it beyond 1000, the whole of the ice will be melted. These extreme cases are easily found; for by Cor. 3, Prop. 16, $w = 19\,W \cdot \frac{1000 - T}{22\,t - 19000}$ when all the mixture is frozen and its temperature 1000; and $w = W \cdot \frac{22000 - 19\,T}{22\,t - 22000}$ when the mixture is completely liquid, and its temperature 1000. Therefore if the given w exceed $19\,W \cdot \frac{1000 - T}{22\,t - 19000}$ and fall short of $W \cdot \frac{22000 - 19\,T}{22\,t - 22000}$, the mixture will be partly liquid and partly solid.

Again, since by Cor. 4, Prop. 4, $\tau = \frac{W\,B\,T + w\,b\,t}{W\,B + w\,b} = \frac{19\,W\,T + 22\,w\,t}{19\,W + 22\,w}$, by substituting for B and b their values found by Cor. 1, Prop. 16, and by supposing there is no solidification or liquefaction arising from the mixture, we shall have $1000 = \frac{19\,W\,T + 22\,w\,t}{19\,W' + 22\,w'}$ on the idea that W' is the quantity in the solid state, and w' that in the fluid state, the mixture being at 1000, the temperature of liquefaction. But there being no increase or diminution of matter, $W' + w' = W + w$, and, therefore, $1000 = \frac{19\,W\,T + 22\,w\,t}{19\,(W + w) + 3\,w'}$, and $w' = \frac{19\,W\,T + 22\,w\,t - (W + w)\,19000}{3000}$ the quantity of water in the mixture. Consequently $W + w - w'$ is the quantity of ice in the mixture.

Cor.—Hence if w' is equal to or greater than $W + w$, all the mixture is liquid; and if it come out nothing, or negative, all the mixture is solid. By putting $w' = 0$, we shall get $w = 19\,W \cdot \frac{1000 - T}{22\,t - 19000}$; and by putting $w' = W + w$, we shall get $w = W \cdot \frac{22000 - 19\,T}{22\,t - 22000}$, which theorems coincide with those deduced in the first part of the present proposition.

PROP. XVIII. THEOR. XIV.

If a given weight, w, of water at a given temperature, t, be thrown on or mixed with an indefinite quantity of ice at the temperature of its liquefaction, I say that the weight of ice melted, no other cause interfering, will be equal to $\frac{t - 1000}{3000}\,22\,w$.

This theorem I have given without demonstration in the Scholium to Prop. 1 of the present paper, or page 102 of the present volume of the *Annals*. I might with propriety have deduced it in a cor. to the preceding Prob. but in consequence of its involving the theory of the Calorimeter, the invention of those celebrated philosophers MM. Lavoisier and Laplace, I have made it the subject of a separate proposition.

In whatever way we conceive the water to be applied to the ice, whether it be scattered over an indefinite portion of it, or communicate in a body with one particular part, it has no influence on the quantity of ice liquefied; for the quantity melted will be evidently proportional to the amount of temperature the whole water can part with to reduce it to the temperature of liquefaction; that is, to the excess of the water's temperature above 1000, and the quantity of the water conjointly. Hence the temperature of the water will have no effect on any other part of the ice but that which it absolutely liquefies; and, therefore, we may, in pursuing the consequences of the mixture, imagine the water to be mixed with as much ice only as it really liquefies, and totally disregard its connexion with the rest. Let W' be the quantity it liquefies; then $19000\,W' + 22\,w\,t$ is equal to the united temperatures of the ice and water before the mixture, and $22000\,(W' + w)$ the same thing after the mixture. But nothing being gained or lost, these quantities must be equal; that is $22000\,(W' + w) = 19000\,W' + 22\,w\,t$: whence $W' = 22\,w\,\frac{t - 1000}{3000}$. Q. E. D.

Cor. 1.—The application of this theorem to the determination of the quantity of ice melted in the calorimeter, I have already given, by calculating the effect which an equal weight of water at the given temperature would have, and then augmenting or diminishing this effect in the proportion of the baromerin of water to that of the body to be tried; but the theorem may be expressed in terms of the weight, baromerin, and temperature of the body itself, by only changing the weight and baromerin of the water for the like things of the body. Thus if the weight of the body be Q, and its baromerin q, that of water being 22, we shall have $W' = Q\,q\,\frac{t-1000}{3000}$.

Cor. 2.—Hence conversely the baromerin of any body may be discovered by the weight of ice it will melt in the calorimeter. For by the last Cor. we get $q = \frac{3000\ W'}{Q\,(t-1000)}$, which is a general expression.

Scholium.

Because by "specific caloric" is meant the ability of bodies to affect the temperature of another under equal circumstances, reckoning in Fahrenheit degrees with our English philosophers; and because these degrees within ordinary limits are sensibly proportional to our units of true temperature, it is plain that the ratio of our baromerins, determined from the preceding theory, ought to coincide very nearly with the ratio of the "specific calorics." Let us see by the calculation of one of Lavoisier and Laplace's experiments how far this will be the case. They put a piece of iron-plate, at the temperature of 97·5° centigrade, weighing 3·77264 kilogrammes into the calorimeter, and at the end of 11 hours found its temperature was reduced to 0° cent. and that ·542004 kilogrammes of ice were melted. By the tables, p. 515, vol. i. Murray's Chemistry, 97·5° cent. = 207·5° Fahr. = 1168·6 true temperature. Therefore, $t = 1168{\cdot}6$, $W' = {\cdot}542004$, and $Q = 3{\cdot}77264$; whence by Cor. 2, of this Prop. $q = \frac{3000 \times {\cdot}542004}{3{\cdot}77264 \times 168{\cdot}6} = 2{\cdot}55635$ the baromerin of iron plate; the baromerin of water being 22. Dividing the baromerin thus found by 22, it gives ·116189 for the baromerin of iron plate, that of water being 1. MM. Lavoisier and Laplace calculated the "specific caloric" of this body for the same unity at ·11051, which very nearly coincides with our baromerin. A like accordance would, I have no doubt, be found in other cases, but at present this must suffice.

Prop. XIX. Theor. XV.

In a former part of this paper I have shown that water may be cooled below the temperature of its liquefaction without solidifying, but that on agitating it, a part becomes frozen, and the temperature of the whole rises to 1000. Let w be the weight of a given quantity of water cooled down to t, and let it be shaken; then the weight of the quantity frozen will be equal to $\frac{1000-t}{3000}\,22\,w$.

Put W′ to denote the weight of the water frozen, and we shall have $w - W'$ for the weight of the water not frozen. Since the temperature after agitation rises to 1000, the aggregate temperature after the agitation is $1000 \{19\,W' + 22\,(w - W)\} = 1000\,(22\,w - 3\,W')$. But the aggregate temperature before agitation must have been $22\,w\,t$; and because no temperature is gained or lost by agitation, these two aggregates must be equal. Therefore $1000\,(22\,w - 3\,W') = 22\,w\,t$, and $W' = 22\,w \times \frac{1000 - t}{3000}$.

Scholium.

Dr. Thomson, p. 54, vol. i. of the sixth edition of his Chemistry, says: "I find that when water is cooled down to 22° Fahr. very nearly $\frac{1}{14}$th of the whole freezes by agitation;" "when the previous temperature is 27°, about $\frac{1}{28}$ of the whole freezes." At 22° Fahr. $t = 989{\cdot}5$; therefore, putting $w = 1$, our formula gives $W' = 22\,\frac{10{\cdot}5}{3000} = \frac{201}{3000} = \frac{1}{13}$ very nearly, which differs from $\frac{1}{14}$ by $\frac{1}{182}$d part of the whole mass of the water. Again 27° Fahr. gives $t = 994{\cdot}8$; and, therefore, $W' = 22\,\frac{5{\cdot}2}{3000} = \frac{1144}{30000} = \frac{1}{26{\cdot}22}$ very nearly, or $\frac{4}{105}$, which exceeds $\frac{1}{28}$ by $\frac{7}{2940}$ or $\frac{1}{420}$th of the water operated on. Had Dr. Thomson, therefore, experimented with a pint of water, or 20 oz. avoirdupois, which I expect is a greater quantity than he employed, an error in the first instance of only $\frac{1}{9{\cdot}1}$ oz. and in the second of $\frac{1}{21}$ oz. in the determination of the quantity of water frozen would make his experiments coincide with our theory. A less quantity of water experimented with would give proportionably less differences. With experiments of this kind there must be considerable difficulty in effectually preventing the fluid part at the separation from taking away with it some of the light, loose, incoherent solid which is formed; and hence it must usually follow that the theory will give somewhat greater quantities than experiment, as we see it happens in the present cases.

If we could cool the water down so low that the whole of it would congeal on agitation, we shall have $W' = w$, and $t = 1000 - \frac{3000}{22} = 863{\cdot}6$. Therefore could we cool water without freezing to the true temperature 863·6, which corresponds with − 90° Fahr. it would become one solid mass on agitation. Dr. Thomson calculates from his experiments that this would take place at − 108°. It is curious that the number − 90° Fahr. coincides with the greatest cold yet produced by mixing eight parts of snow with ten parts of sulphuric acid diluted with half its weight of water, the constituents being first at the temperature − 68° Fahr.

Prop. XX. Prob. V.

Let it be required to determine the ratio of the baromerins of water and vapour, the result of some experiment on the mixture of certain portions of these bodies being given.

We may now be said to have arrived into the regions of experimental inconsistency and error. After what we have already done on this subject, there can be no difficulty in the physical or mathematical part of our present inquiries; but the misfortune is we know not what or whose experiments to prefer. In no one subject, except, perhaps, in that of "capacities," do the experiments of philosophers exhibit such incongruous and discordant results, as on their favourite and much cultivated doctrine of "latent heat;" and particularly on that part of it which is of by far of the most practical consequence, the determination of the "latent heats" of vapours. A fair specimen of these discordancies may be seen in the following table:

Authors.	Latent heat of aqueous vapour.
Black	800° Fahr.
Watt	950
Lavoisier.	1000
Southern.	945
Clement and Desormes	990
Rumford.	1021
Thomson.	1016
Ure	888

N.B. Dr. Ure computes the latent heat of water to be 967°; but that his experiment gives only 888°.—See note to Prop. 22.

The number here set against Dr. Thomson's name I have copied from page 70 of the first volume of his Chemistry; but I cannot tell whether it be the result of experiment or calculation. If it be derived from experiment, the circumstances are by far the most judiciously chosen of any that I have seen. In general, philosophers have sought to determine the "latent heat" of vapour by the condensation of a very small quantity of vapour on a very considerable quantity of water. By this process, the rise of temperature which the condensation occasions is very inconsiderable, and consequently a very trifling error in measuring this rise exposes the determination of the numerical value of its cause to material errors. This objection applies with considerable force against the experiments of Count Rumford, where the augmentation of temperature is under 13° Fahr. and with much greater against the experiment of Dr. Ure, who had an increase of only $6\frac{1}{3}$°. In the case mentioned by Dr. Thomson, the rise is no less than $116\frac{3}{5}$° Fahr. which is sufficient, if it be the result of correct experiment, to enable us to determine, with all reasonable precision, the elements of the problem in question. For this reason, and because the result nearly coincides with that given by Count Rumford, who had evidently conducted his experiments with consummate skill and precaution, and nearly with that by MM. Lavoisier and Laplace, I have preferred it for the investigation of the numbers of the present problem. Besides it appears to me, that had Dr. Ure taken into account the matter in his glass basin and condensing globe, he would have brought out a much higher number than he has, if not very nearly the same as Rumford, or Lavoisier and Laplace; and, therefore, I look on that philosopher's experiment as corroborating the higher numbers of these.

By a similar train of reasoning to that which I have pursued in Prop. 16, we obtain the equation $(w+w')\,b\,\tau' = w\,b\,t + w'\,b'\,t'$ when all the vapour is condensed, and $(w + w')\,b'\,\tau' = w\,b\,t + w'\,b'\,t'$ when all the water is converted into vapour; taking w, t, b, to represent the same things as in the cited Prop. 16, namely, the weight, true temperature, and baromerin of the water, and w', t', b', the same things respectively of the vapour; and taking τ' to represent the true temperature of the mixture. From the first of these equations, we have $b : b' :: w'\,t' : (w + w')\,\tau' - w\,t$, and from the second $b : b' :: (w + w')\,\tau' - w'\,t' : w\,t$.

Cor. 1.—In the case stated by Dr. Thomson, to which I have already alluded, it is said that one part by weight of steam at 212° Fahr. mixed with nine parts of water at 62°, gives a mixture of 178·6°, the steam being all condensed. Here $t = 1030{\cdot}8$, $\tau' = 1142{\cdot}5$, $t' = 1172{\cdot}6$, $w = 9$, and $w' = 1$; and, therefore, $b : b' :: w'\,t' : (w + w')\,\tau' - w\,t :: 1172{\cdot}6 : 1142{\cdot}5 - 9277{\cdot}2 :: 1172{\cdot}6 : 2147{\cdot}8 :: 1 : 1{\cdot}83166 :: 6 : 11$.

We can now, substituting these numbers, compare the theorem with experiment. If we take the case from which we have computed them, the middle temperature will come out 178·8° Fahr. or ·2° higher than 178·6° from which the numbers were derived. This difference arises from the numbers 11 and 6 having a greater ratio than 2147·8 and 1172·6.

Dr. Ure distilled 200 grains of vapour in 32340 grains of water at 42·5°, and found the result 49°. Here we have $T = 1010{\cdot}9$ and $t = 1172{\cdot}6$; and because $\frac{W}{w} = \frac{32340}{200} = 161{\cdot}7$,

we have $$\tau = \frac{161{\cdot}7 \times 1010{\cdot}9 \times 6 + 1172{\cdot}6 \times 11}{6 \times (161{\cdot}7 + 1)} = 1017{\cdot}9,$$ which gives

49·3° Fahr. that is, $\frac{3}{10}$ of a degree above what Dr. Ure observed it to be. In this experiment of Dr. Ure, it is to be observed that he does not note the height of the barometer at the time of the experiment, a circumstance which he must be aware would influence the result of the calculation, perhaps indeed almost sufficient to annihilate even the trifling difference which there is between the theory and experiment. My theorem presupposes the barometer to have been at 30 inches. The same omission likewise exists in the following experiments of Count Rumford; and, therefore, I must assume the same, as in the experiment of

Dr. Ure, that the ebullition actually took place at 212° Fahr. the barometer being 30 inches.

In one experiment Rumford distilled 29·61 French grammes of vapour into 2781 grammes of water at 55° Fahr. and found the mixture 67·5° Fahr. These data give $W = 2781$, $w = 29{\cdot}61$, $T = 1023{\cdot}7$, and $t = 1172{\cdot}6$. To shorten the calculation, we may put $\frac{W}{w} = \frac{2781}{29{\cdot}61} = 93{\cdot}921$, and then we shall have $\tau = \frac{1023{\cdot}7 \times 93{\cdot}921 + 1172{\cdot}6 \times \frac{11}{6}}{93{\cdot}921 + 1} = 1035{\cdot}6$, which corresponds with 66·8° Fahr. ·7° below the observation.

In another experiment he distilled 24·40 grammes of vapour on the same quantity, 2781 grammes of water, at $57\frac{1}{4}$° Fahr. and found the temperature raised to 67·5° Fahrenheit. Hence $\frac{W}{w} = \frac{2781}{24{\cdot}40} = 113{\cdot}9754$, $T = 1026$, and $t = 1172{\cdot}6$. Therefore, $\tau = \frac{1026 \times 113{\cdot}9754 + 2149{\cdot}76}{113{\cdot}9754 + 1} = 1035{\cdot}8$, or 67° Fahr. that is ·5° beneath the experiment. The following table contains these calculations collated with the experiments:

Authorities.	Quantities mixed, with their temp. in Fahr.				Temperature of the mixture by		Excess of theory above experiment.	
	Vapour.		Water.					
	Weight.	Temp.	Weight.	Temp.	Exper.	Theory.		
Thomson	1·0	212°	9	62·0°	178·6°	178·8°	+	·2°
Ure. . . .	200·0	212	32340	42·5	49·0	49·3	+	·3
Rumford.	29·61	212	2781	55·0	67·5	66·8	−	·7
Ditto. . .	24·40	212	2781	57·25	67·5	67·0	−	·5
						M. diff.	−·175	

Philosophers will perceive from this and the other specimens I have given, that we are not more unfortunate in our investigations of the mathematical laws of "Latent Heat" than we have been in any of the rest of our inquiries.

Cor. 2.—Substituting the numbers thus found for b and b', we shall have $\tau' = \frac{6\,w\,t + 11\,w'\,t'}{6\,.\,(w + w')}$ all the vapour being condensed, and $\tau' = \frac{6\,w\,t + 11\,w'\,t'}{11\,.\,(w + w')}$ all the water being vaporised.

By the former of these theorems, if we take $w = 100$, $w' = 1$, $t =$ (201·2° Fahr.) ·1163, and $t' = 1172{\cdot}6$, we get $\tau' = 1172{\cdot}7$, or 212° Fahr.; that is, 1 oz. of steam at 212° Fahr. condensed in 100 oz. water at about 201° Fahr. will raise the temperature of the water to 212° Fahr. the same as I have stated p. 102 of the present volume of the *Annals*.

Cor. 3.—Because when the vapour is all condensed, and the water beneath 1172·6, true temp. $\tau' = \frac{6\,w\,t + 11\,w'.t'}{6\,(w + w')}$, we obtain $w' = \frac{(\tau' - t)\,6\,w}{11\,t' - 6\,\tau'}$; that is, a quantity of water being given, a quantity of vapour may be found, whose temperature is known, which, condensed on the water, shall raise its temperature from any one given temperature to any other under 1172·6, or the term of ebullition, according to the pressure under which we operate.

Cor. 4.—And since by the same formula we get $w = w' \frac{11\,t' - 6\,\tau'}{(\tau' - t)\,.\,6}$, we are enabled to determine the quantity of water, which, at a given temperature, shall condense with a given increase of temperature a given quantity of steam whose temperature is known.

If we put $\tau' = 1172{\cdot}6$, the theorem $w = \frac{11\,t' - 6 \times 1172{\cdot}6}{(1172{\cdot}6 - t)\,.\,6}\,w'$ determines the quantity of water at a given temperature which it will just require to condense the whole of a given portion of steam at a given temperature.

Cor. 5.—By the second theorem in Cor. 2, we find $w = \frac{(t' - \tau')\,.\,11\,w'}{11\,\tau' - 6\,t}$. Therefore the quantity and temperature of the steam being given, the quantity of water at a given temperature may be found, which, introduced into the apparatus, shall be wholly converted into steam, and the temperature of the mixture be reduced to a given temperature.

Scholium.

The numbers 6 and 11, that I have found for the proportion of the baromerins of water and vapour, will also, like those for water and ice, give the ratio of the relative powers of equal weights of water and vapour at 212° Fahr. to effect changes in the temperature of any other body; or, in the language of the old doctrine, they exhibit the ratio of the "calorific capacities." Therefore if the capacity of water be 1, that of vapour must be $\frac{11}{6} = 1{\cdot}83$, which differs from the experimental determination 1·55 of Dr. Crawford by a quantity which every philosopher will allow to be much within the limits that probability would assign to the errors of experiment.

From the baromerin of vapour thus determined, we might easily deduce that of any gas whatever, supposing the vapour and gas respectively homogeneous. For this purpose, the theorem I published in the *Annals* for July, 1816, is serviceable. By that theorem, it appears that the baromerins in any homogeneous airs are reciprocally proportional to the square roots of the specific gravities; and hence the baromerin of water being unity, that of any homogeneous gas (taking for granted that vapour itself is homogeneous) is equal to $\frac{11}{6}\sqrt{\frac{V}{G}}$; in which V denotes the specific gravity of vapour, and G that of the gas in question.

Though this formula must be considered as mathematically true only in the case of perfect homogeneity, yet it would not differ very materially in compound cases; and, therefore, in any airs with which we are acquainted.

Those barometrins, as I have repeatedly stated, are proportional to what is understood by the "calorific capacities or specific heats." The discordance, however, in the determinations of the "specific heats" of gases by different philosophers is so great, that it is absurd, if not ridiculous, to attempt to compare the theory with them. For instance, Crawford, who is conceived by the British philosophers to be the most accurate in his experiments of this kind, makes the "specific heat" of hydrogen 21·4, while De Laroche and Berard, who obtained the prize of the Royal Institute of France for their experiments on this subject, make it only 3·2936, which is something better than a seventh part of Crawford's. Again, by the former, oxygen comes out 4·749, and by the latter, ·2361; so that here Crawford's number is not only six or seven times, as in the instance of hydrogen, as great, but upwards of 20 times. Our theorem gives 1·375 for oxygen, and 5·50 for hydrogen; but philosophers will have the goodness to recollect, that these numbers rest on the supposition of the individual homogeneity of vapour, oxygen, and hydrogen. A theorem might be easily given for any degree of complexity or compoundness of composition in the airs, from which some interesting conclusions may be drawn; but these things I shall reserve for another opportunity. One point may be observed, in which our theory agrees with the general results of both parties, namely, that the "specific heats" of the lighter airs exceed those of the heavier.

If we wish to have a theorem which will include the laws of any changes on the body in its three states at once, we can easily obtain it. For since by Cor. 1, Prop. 16, when $B = 19$, $b = 22$; and since by Cor. 1 of the present Prop. when $b = 6$, $b' = 11$, we have $B = 19$, $b = 22$, and $b' = 22 \times \frac{11}{6} = 40\frac{1}{3}$; or if we prefer whole numbers, $B = 57$, $b = 66$, and $b' = 121$. From these data we get the general equations $\tau = \frac{19\,T\,W + 22\,t\,w + 40\frac{1}{3}\,t'\,w'}{19\,(W + w + w')}$; or $= \frac{19\,T\,W + 22\,t\,w + 40\frac{1}{3}\,t'\,w'}{22\,(W + w + w')}$; or $= \frac{19\,T\,W + 22\,t\,w + 40\frac{1}{3}\,t'\,w'}{40\frac{1}{3}\,(W + w + w')}$, according as the mixture is wholly solid, wholly fluid, or wholly vapour. Several interesting phænomena connected with these general equations brevity obliges me to leave unnoticed.

Prop. XXI. Prob. VI.

The temperature of a given weight of water being known, to determine the quantity which must be evaporated to reduce it to a given temperature.

Denoting, as before, the weight of the water by w, and putting w' for the quantity of vapour liberated, we have $\tau' = \frac{6\,w\,t}{6\,(w - w') + 11\,w'} = \frac{6\,w\,t}{6\,w + 5\,w'}$; whence $w' = \frac{(t - \tau')\,6\,w}{5\,\tau'}$. Q. E. I.

Cor. 1.—Conversely the quantity lost by evaporation, and the primitive temperature, being given, the reduced temperature may easily be found; for it will be simply $\tau' = \frac{6\,w\,t}{6\,w + 5\,w'}$.

Cor. 2.—By this theorem we can also determine the effect which a given diminution of compression will have on the temperature of water by suffering a portion of its vapour to escape. From the views I have taken in a preceding part of this paper, the greatest temperature water can endure, under a certain compression, is its temperature of ebullition. If the temperature be attempted to be raised above this, ebullition takes place, and the decomposition proportions itself to the accession of temperature, so as to keep the temperature at about the same. Therefore, if any part of the compression be removed, and the previous temperature was higher than that of ebullition corresponding to the reduced compression, ebullition will immediately ensue, and the former temperature descend to the latter. In general, and particularly in high temperatures, this diminished temperature, or temperature of ebullition, will not, as I have shown, materially differ from the corresponding temperature of tension; so that we may hence substitute the one for the other without committing any great errors. By Prop. XV. if E equals the tension or compression, and τ' the temperature, $E = 30\,\{\cdot00278313\,\tau' - 2\cdot263714\}^2 = \phi\,\tau'$; and, therefore, $\tau' = \phi^{-1}\,E$. Consequently the new temperature is immediately known from the compression.

The weight of vapour likewise lost is given in the same terms; for $w' = \frac{(t - \tau')\,6\,w}{5\,\tau'} = \frac{(t - \phi^{-1}\,E)\,6\,w}{5\,\phi^{-1}\,E}$.

Cor. 3.—This theorem, and some of the preceding in the present paper, enable us to develop several curious things respecting cooling and diminution by evaporation, in functions of themselves and the time; but these things leading to very extensive inquiries, I propose to consider them at another opportunity. However, any one who wishes to pursue the subject will find many of the phænomena involved in the equation $-dT = \frac{6\,w\,t}{5\,\tau'^2\,\phi^{-1}\,\tau'}\,d\tau'$; in which T is the time of departure from the primitive temperature t; w, as before, the primitive weight of water; and τ' the indeterminate temperature.

Scholium.

I have sedulously looked over all the authors I have at hand on the subject in order to procure the results of some experiments to compare the present proposition with, but have not been fortunate enough to meet with more than one. Mr. Watt having heated a quantity of water in a Papin's digester to 400° Fahr. suddenly opened the vessel, when about a fifth of the water rushed out in vapour, and the temperature of the rest sunk to 212°. By this experiment, $\tau' = 1172{\cdot}6$, and $t = 1329{\cdot}2$; and putting $w = 1$, we have $w' = \frac{1329{\cdot}2 - 1172{\cdot}6}{5 \times 1329{\cdot}2} 6 = \frac{940}{6646}$ or $\frac{1}{7}$ nearly. This differs from $\frac{1}{5}$ by a $\frac{1}{17\frac{1}{2}}$th of the whole quantity of water; a difference which philosophers will allow to be sufficiently minute for an experiment of this kind. It is observable that the experimental loss exceeds the computed, which is just what we should expect from the manner of making the experiment; for as the vapour rushes out with very great violence, it is highly probable it will force out with it more water, than, if converted into vapour, would be sufficient to reduce the temperature to 212°.

The theorems demonstrated in the present Proposition and its Corollaries furnish us with very easy means of experimentally examining the effects of what is called radiation either to or from water, under peculiar circumstances of temperature, &c. For the quantity evaporated being known, its individual effect on the temperature becomes known, and hence the effect of radiation.

Prop. XXII.* Prob. VII.

"Let two vessels communicating with a boiler be filled with steam of any temperature, and heated themselves to the same degree, say 212°. Let one be detached from the boiler, and let the other continue to communicate with it. Let the elastic force of the steam be doubled, trebled, &c. in both; required the corresponding densities of the steam in each vessel, one of course being constant; their capacities for heat, and quantities of latent heat above what is present at the temperature of 212°."

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* This problem, or rather problems, was proposed to me in a very early stage of my inquiries into the laws of vapours by a gentleman, an able mathematician, under circumstances, which, to prevent misconception of their cause on one side, and the motives on the other, it may be necessary to mention. Being one afternoon at the Rev. H. S. Trimmer's, I informed him, that some ideas had just before occurred to me respecting the difference between vapours and permanent airs or gases, which gave me hopes I should, at some period, be able to bring the phænomena of vapours, and with them the theory of the steam-engine, within the pale of mathematical laws. During this conversation, I happened to mention that the gentleman alluded to had told me about 15 months before, that he had a problem connected with steam which he wished to get solved, and which he had proposed to some of the most conversant in these matters for that purpose, but had not received any satisfactory answers. At the same time I observed, that I thought, if I had and knew what this problem was, I could solve it. Mr. Trimmer immediately proposed that I should write to the gentleman, and undertake the solution, and, without scarcely waiting for my reply, placed before me a pen, ink, and paper. While writing this letter, and keeping up a conversation with Mr. and Mrs Trimmer, I inadvertently gave the above gentleman not only liberty to send me the problem in question, but any other of the kind he might choose. The disadvantageous situation in which this offer placed me, and the power it gave to a very able man to put my skill to a severe trial, were immediately perceived, but relying on the accuracy of my principles, and the facility with which things flow from them, I determined to send the letter off as it was rather than take the trouble to write another. The consequence was, that about a fortnight afterwards I received the above problems, which I at once saw far transcended the powers of any thing yet published to resolve. However, as soon as I had read them, I felt persuaded my theory would help me out; and

Case 1.—Let it be required to determine the ratio of the capacities. Suppose F_1, $F+n$, F_1, F_1+n, to denote any Fahrenheit temperatures, and t, t_1, t_2, t_3, the corresponding true ones. Then because $t : t_1 :: \sqrt{F+448} : \sqrt{F+n+448}$, and $t_2 : t_3 :: \sqrt{F_1+448} : \sqrt{F_1+n+448}$, we have $t_1 - t : t_3 - t_2 :: \sqrt{F+n+448} - \sqrt{F+448} : \sqrt{F_1+n+448} - \sqrt{F_1+448}$; and, therefore, $\frac{t_3 - t_2}{t_1 - t} = \frac{\sqrt{F_1+n+448} - \sqrt{F_1+448}}{\sqrt{F+n+448} - \sqrt{F+448}} = \sqrt{\frac{F+448}{F_1+448}}$ when $n = o$, which is, therefore, the capacity at F_1, that at F being unity.

The above theorem gives the ratio of the capacities in terms of the Fahrenheit temperatures; but as the proposer requires it in terms of the elasticities, and of the capacity at 212°, that also may be given.

First, in the case of the detached boiler. By Prop. 8 of my former paper, $E = e\,\frac{F_1+448}{F+448} = \frac{F_1+448}{22}$, accounting $e = 30$ when $F = 212°$. Hence by substitution

$$\frac{t_3 - t_2}{t_1 - 1} = \frac{\sqrt{22\,E+n} - \sqrt{22\,E}}{\sqrt{660+n} - \sqrt{660}} = \sqrt{\frac{30}{E}}$$ the capacity sought, when $n = o$.

Secondly, in the case of the attached boiler, we have Prop. 21, Cor. 2, true temperature $= \phi^{-1}E$; and, therefore, $F_1 + 448 = (\phi^{-1}E)^2 \times \frac{660}{1375000} = (\phi^{-1}E)^2 \times \frac{3}{6250}$, when $F = 212°$, and the corresponding elasticity $= 30$. Hence

$$\frac{t_3 - t_2}{t_1 - 1} = \frac{\sqrt{(\phi^{-1}E)^2 \cdot \frac{3}{6250} + n} - \sqrt{(\phi^{-1}E)^2\,\frac{3}{6250}}}{\sqrt{660+n} - \sqrt{660}} = \frac{1172{\cdot}6}{\phi^{-1}E},$$ the capacity sought, when $n = o$.

Case 2.—To determine the latent heats in terms of the elasticities. By Prop. 20, Cor. 2, we have $\tau' = \frac{6\,w\,t + 11\,w'\,t'}{6\,(w+w')}$ all the steam being condensed; and by Prop. 1, Cor. 2, we have $\tau =$

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though I had not then solved the principal part (our 15th Prop.) I replied that as soon as I was released from a mathematical investigation of Mr. Dalton's laws of evaporation, which I was then busily trying to discover and arange for the press, I would endeavour to send him the solutions he desired.

$\frac{wt + w't'}{w + w'}$, supposing w' to be water instead of steam. Therefore, $\tau' - \tau = \frac{5 w' t'}{6 (w + w')}$ is the rise of temperature due to the condensation of w' weight of vapour; and consequently $\frac{5 t'}{6 (w + 1)}$ is the rise due to the condensation of unity weight of steam. But this condensation raises not merely w weight of water from τ to τ', but $w + 1$ weight. Whence the rise of unity weight would be $\frac{5 t'}{6}$, which expresses the effect of condensation in terms of the true temperature. Consequently in the detached boiler the effect in units of true temperature $= \frac{5863 \sqrt{E}}{6 \sqrt{30}}$, and in the attached boiler it $= \frac{5}{6} \cdot \phi^{-1} E$; accounting E the elasticity. And if we suppose the effect of condensation at 212° Fahr. = 1, in the former case it will $= \sqrt{\frac{E}{30}}$, and in the latter $\frac{\phi^{-1} E}{\phi^{-1} 30}$ or $\frac{\phi^{-1} E}{1172 \cdot 6}$.

To express the same things in Fahrenheit degrees, we may multiply each expression by $\frac{F_1 - F}{\tau' - \tau} = \frac{180}{172}$ nearly, supposing F_1 the Fahr. degree corresponding with τ', and F the same thing corresponding with τ; and the products will nearly give the latent heats according to the common views.

If it be required to give a correct expression in terms of Fahrenheit, that also may be done. Let us suppose the weight of vapour = 1, that of water being w; and let F, F_1, and F_2, be the Fahrenheit temperatures of the water, vapour, and mixture. Suppose also that F_3 is the Fahrenheit temperature which would result from mixing one of water instead of steam at F_1 with w of water at F. Then $F_2 - F_3$ is manifestly the increase of temperature occasioned by the condensation, and is, therefore, the whole latent heat developed by the condensation of unity weight of vapour. But this latent heat acts not on the w weight of water only, but on the whole mass of water and condensed steam;* for if there were no latent heat developed, the whole mass $w + 1$ would remain at F_3, whereas it is found at F_2. Hence $(F_2 - F_3) \cdot (w + 1)$ expresses the Fahrenheit degrees of latent heat contained in a unity of vapour at F_1.

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* It is curious that this circumstance has escaped the notice of Dr. Ure in his calculation of the latent heat of water, in the Transactions of the Royal Society for 1818, p. 388. He computes the latent heat from the formula $(F_2 - F_3) w$, instead of $(F_2 - F_3) \cdot (w + 1)$. But just above, in the same page, another error of a more extraordinary kind appears. The Doctor says, "$\frac{84}{161 \cdot 7} = \cdot 52$ is the elevation of temperature which 1 of water at 212° would occasion on 161·7 of water at 42·5°." Now the truth is $\frac{212 - 42 \cdot 5}{161 \cdot 7 + 1} = \frac{169 \cdot 5}{162 \cdot 7} = 1 \cdot 04$ is the proper elevation according to the old doctrine, or double what Dr. Ure has computed it to be. These errors rectified give the Doctor's latent heat of water $\overline{6 \cdot 5° - 1 \cdot 04°} \times 162 \cdot 7 = 888\frac{1}{3}$, instead of 967°. Whether like errors pervade the other latent heats he has there given, I cannot say; I have not

Now by Cor. 2, Prop. 20, $F_2 = \frac{48}{100000} \left(\frac{t\,w + \frac{11}{6}\,t'}{w + 1}\right)^2 - 448$

and $F_3 = \left(\frac{t\,w + t'}{w + 1}\right)^2 \cdot \frac{48}{100000} - 448$, therefore $(F_3 - F_2) \cdot (w + 1)$ $= \frac{t\,w + \frac{17}{12}\,t'}{w + 1} \times \cdot 0008\, t'$, which is a general expression for the latent heat agreeable to the usual ideas. If to satisfy the proposer we wish to have the expression for the detached boiler in terms of the elasticity E, let us put t the lower temperature equal to 1000, and the elasticity of the vapour supposed to follow the ordinary laws of gases at that temperature also equal to the same quantity 1000. Then $t' = 10 \times \sqrt{10\,E}$, and the latent heat is equal to $\frac{100\,w + \frac{17}{12}\sqrt{10\,E}}{w + 1} \times \cdot 08 \sqrt{10\,E}$.

In the case of the attached boiler we have $t' = \phi^{-1}\,E$ and $t = \phi^{-1}\,e$; and, therefore, the latent heat $= \frac{w \cdot \phi^{-1}\,e + \frac{17}{12} \cdot \phi^{-1}\,E}{w + 1}$ $\times \cdot 0008\, \phi^{-1}\,E$.

From both of these expressions, we perceive the proportion of water to steam has an influence in the determination of the quantity of latent heat which may be one cause of the discordancies in the experimental results of different authors.

Case 3.—To determine the densities in terms of the elasticities. One of the densities, that of the attached boiler, as the proposer observes, will remain the same; we have, therefore, only to determine the other. Putting a for the arithmeridone, we have, by the theorem, p. 56, *Annals* for July, 1816, $E \propto \frac{t^2\,a^2}{v\,w} \propto t^2\,w$, because v is constant, and $a \propto w$; therefore, $w \propto \frac{E}{t^2}$, and $w = \cdot 4545 \times \frac{E}{30} \times \frac{1375000}{(\phi^{-1}E)^2} = 20831 \cdot 25 \times \frac{E}{(\phi^{-1}\,E)^2}$ which expresses the value of the specific gravity or density of vapour in terms of the elasticity E as the proposer requires, the elasticity at 212° Fahr. being 30, and specific gravity ·4545.

Scholium.

If in the leading theorem of the first case we put $F = 32°$, $F_1 = 122°$, and $n = 90° = F_1 - F$ or $212° - F_1$, we get

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made the calculations. My design in pointing out these, and other things I have had occasion to allude to, is not, I assure the Doctor, to depreciate the merits of his paper, to which he must perceive I have been much indebted, but merely to call his attention to a recomputation of the results he has given; for from no person, it appears to me, can the rectification of oversights come with so good a grace as from their author.

$\frac{t_3 - t_2}{t_1 - t} = \frac{\sqrt{660} - \sqrt{570}}{\sqrt{570} - \sqrt{480}} = \frac{1 \cdot 81579}{1 \cdot 96577} = \frac{140}{151}$ nearly. So that from this it appears, that the capacity as we ascend in the scale of temperature is decreasing, and that the mean capacity between 122° and 212° is to the mean capacity between 32° and 122° as 140 to 151; De Luc found the ratio to be as 14 to 15 in water, and by my experiments the same ratio holds good in mercury.

By the theory I have expounded, the capacity, as philosophers call it, of every body ought to diminish with the ascent of temperature, provided no corpuscular change takes place in the body; and taken at the temperatures of water freezing and boiling, it ought to have a ratio of 1172·6 to 1000, or of 7 to 6 nearly. The experiments of Mr. Dalton, Dr. Ure, De Luc, and myself, confirm the general truth of this law in water and mercury, and the experiments of the two latter the correct ratio. The experiments of MM. Dulong and Petit give a different result. They find the capacity of mercury and a few other bodies to be slightly increasing; but as these philosophers have not published the details of their experiments, it is impossible to say to what sources of error they may have been exposed. Here are at least four testimonies to one against them; and I have no doubt, if the experiments are carefully repeated, and no corpuscular change takes effect, that the results, and even the numerical values, will be as I have stated, and not in one kind of bodies only, but in every kind.

In the body of the proposition, I have stated that the proportion of water to the vapour experimented with ought to have an influence in the resulting quantity of latent heat. In general I find that the less the ratio of the water to the vapour, the less comes out the value of the quantity of latent heat. Suppose, for instance, that 1 part of vapour at 243·20° Fahr. or 1200 true temperature, be condensed on 9 parts of water at 32° Fahr. or 1000 true temperature, then, by our theorem, the latent heat would come out 1027·2° Fahr.; whereas if the same temperatures were used, and there was only 1 part of vapour to 99 parts of water, the latent heat would not exceed 966·72° Fahr. which is $58\frac{1}{2}$° below the other result. Here then is a circumstance of which philosophers have never dreamt, and which is nevertheless fully borne out by phænomena. Thus Dr. Ure, by condensing steam on water in the proportion of 1 to 161·7, finds the latent heat to be $888\frac{1}{3}$°; while Count Rumford, by using the proportion of 1 to 114, finds it 1018·6°, and with the proportion of 1 to 96·1, as much as $1023\frac{1}{4}$°, the experiment being conducted in the same way, and with the same apparatus. We have here not only the testimony of the experiments of two philosophers for the truth of our general position, but what is in this case of infinitely greater consequence, the testimony of experiments made by the same individual with the same apparatus.

The numerical value of latent heat is not influenced by the proportion of the quantities of steam and vapour only, but likewise, and even in a more eminent degree, by the temperature. Other things being alike, the higher either or both of the temperatures are the greater will be the quantity of latent heat. If the vapour be $\frac{1}{99}$th of the water and its true temperature 1200, as I have taken it before, that of the water being 1160 or 197·89° Fahr. the latent heat will be 1218·78°. In a preceding example, when the higher temperature and proportion of water to vapour were the same, but the true temperature only 1000, the latent heat appeared not to exceed 966·72°, that is, full 250° lower. Unhappily I know of no experiments with which I can compare this conclusion; for the temperatures of the water in all the experiments I know of are so near together, and the influence of the inequality of weight in the water so decidedly overbalances that of the trifling difference of temperature, that the effect of the latter cannot be detected. There are, however, some philosophers, I have no doubt, whose zeal for the advancement of science will put this to the test of experimental proof, and by this means have the honour of for ever putting at rest the merits of a doctrine which, it has often appeared to me, reflects more credit on Black for boldness of conception than soundness of judgment.

It is proper to observe, before any experiments are made to confirm these inferences, that the values of latent heat deduced from the preceding theorem presuppose the admission of our general law of temperature for simple mixtures. If this be denied, it will require a different expression for the amount of latent heat. For instance, taking from our third table t' to correspond with F_1, and t with F, the general expression for the latent heat in Fahr. will be $\frac{(\frac{11}{3}t't - t^2 - t'^2)w + \frac{85}{36}t'^2}{w + 1} \times \frac{48}{100000}$. If we put in this expression $t' = 1200$ and $t = 1000$ as before, the value of the latent heat when $w = 9$ comes out 1009·92°, and when $w = 99$, it is 947·71°. So that here we have not only the inference verified which I have drawn from my general theory, and confirmed by the experiments of Ure and Rumford, but even verified in a more striking degree; for the difference is here $62\frac{1}{4}$° instead of $58\frac{1}{2}$°.

However, though the proportion of weight has as great an influence in this theorem as in the last or a greater, the variation of temperature has by no means so great. For instance, if we retain $w = 99$ and $t = 1000$ (32° Fahr.) the latent heat will be 978° by putting $t' = 1250$, which corresponds with 302° Fahr. The increase of latent heat, therefore, other things being alike, by putting the vapour at 302° instead of 243°, or 59° higher, is 11°. Now by Mr. Southern's experiments, an augmentation of 66° in the temperature of the steam, that is, from 229° to 295°, gave an increase of 8° in the latent heat. I need make no observation on this new accordance of our theory with observation. Philosophers will, by this unexpected fact perceive with pleasure with what fidelity the results of experiment are represented by our theory, even when warped with the notions of caloric. But striking as the coincidence is, we labour under a disadvantage by not knowing the temperature of the water and its proportion to vapour. Had we these data, there is no reason for doubting but our theory would have made the coincidence even more perfect than it is.

To make this theorem also answer the views of the proposer, we have only to substitute 1000 for t, and $10\sqrt{10\,E}$ for t' in the case of the detached boiler, which gives latent heat =

$$\frac{\left(\frac{11000\,E}{3} - 1000 - E\right) w + \frac{85}{36}\,E}{w+1} \times \frac{48}{100} =$$

$$\frac{\left(\frac{10997\,E}{3} - 1000\right) w + \frac{85}{36}\,E}{w+1} \times \frac{48}{100}$$ taking the elasticity at 32° Fahr. for 1000. In the case of the attached boiler, we have $t = \varphi^{-1} e$ and $t' = \varphi^{-1} E$ which substituted in the formula give latent heat =

$$\frac{\left\{\frac{11}{3} \,.\, \phi^{-1} E \,.\, \phi^{-1} e - (\phi^{-1} e)^2 - (\phi^{-1} E)^2\right\} . w + \frac{85}{36} (\phi_{-1} E)^2}{w+1} \times \frac{48}{100000}.$$

In page 270 of the present volume of the *Annals*, I have given some calculations from a theorem similar to that I have deduced in the third case; but as I have not experimental results, but only calculations from a theory which is found to agree nearly with observation within a certain extent, it will be useless to attempt a further comparison.

(*To be continued.*)

Article IX.

Reply to Mr. Tredgold. By John Herapath, Esq.

(To the Editor of the *Annals of Philosophy.*)

DEAR SIR, *Cranford, Hounslow, Nov. 12, 1821.*

It has ever appeared to me to be a more honourable course, even in unprovoked attacks, to give the writer an opportunity of discovering and correcting his own errors than triumphantly to expose them to animadversion. In your *Annals* for October, I took this course with your correspondent X. and Mr. Tredgold, who had thought proper to attack my writings. I perceived, as I stated, errors and misconceptions in the observations of both, which I wished *them* to rectify rather than myself. X. has taken the hint as I wished and expected; and though he has not perceived in his writings what I had in view, he has nevertheless replied in a manner sufficient to demonstrate, that what I said could not possibly give offence to a man actuated by just and peaceable feelings. Unfortunately Mr. Tredgold has taken the thing in a different way. Hence there is a feeling pervading his last paper in the Philosophical Magazine for October, which I am sorry to perceive; and which I the more regret to see, as from the course Mr. Tredgold has thought proper to pursue, it obliges me to exhibit the merits of his two papers in a light from which I would willingly have kept them.

Mr. T. in his last paper, sets out with a professed attempt to demonstrate, "*that in the direct collision of perfectly hard bodies, the momentum before and after the stroke is the same, when estimated in the same direction.*" This equality of momenta, I believe, was never doubted before, not only "*in the direct collision of perfectly hard bodies,*" but in the direct or oblique collision of bodies of every kind, whether perfectly or imperfectly hard, soft, or elastic. In my theory, as in every other, except I suppose Mr. Tredgold's, it is interwoven with the very elements; and I have re-deduced it from my results in a few instances merely to show that the spirit of my inquiries has not departed from this well-known principle. We shall presently see whether Mr. T. has been very successful in his professed attempt to demonstrate it.

If two perfectly hard balls, A, B, moving towards opposite parts in the same right line with the velocities, V, v, strike one another, then, by Mr. Tredgold's views in his last paper, if A V exceed B v, the velocity of B after the stroke in a contrary direction to that in which it moved before the stroke is $\frac{A\,V - B\,v}{B}$.

"For," says Mr. T. "the intensity of the stroke (which is his tension of the thread) cannot be greater than A V, unless there be a reacting force greater than A V; and since B v is less than A V, the *deficiency* of reaction is A V − B v. Therefore A V − B v is the *momentum communicated* to B; or $\frac{A\,V - B\,v}{B}$ the velocity of B."

I cannot stop to notice as they deserve the curious paralogies in these two short sentences; I shall, therefore, merely show how well this inference demonstrates the problem he intends it to prove. Because A V − B v is the "*deficiency* of reaction," it is, by Mr. T.'s account, the motion lost by A; therefore, $A\,V - (A\,V - B\,v) = B\,v$ is the motion of A after collision in the same direction in which it was moving before the collision. But Mr. T. tells us the motion of B in the same direction after collision is A V − B v, and, consequently, the sum of these motions is $A\,V - B\,v + B\,v = A\,V$. Now the aggregate motion in the same direction before collision by his assumption was A V − B v. Therefore the sum A V of the motions after the stroke exceeds the aggregate motion A V − B v in the same direction before the stroke, by the entire motion B v; yet he intended to prove these two motions equal.

This, the scientific world will perceive, is Mr. Tredgold's grand effort, which "strikes at the root of Mr. Herapath's system, and overturns all his conclusions." Let us turn to his first paper in the Phil. Mag. for Aug. and I think we shall find something there which will improve the specimen I have already given.

"By examining," says Mr. T. in a note p. 132, "the simple case (he alludes to bodies moving towards each other with equal opposite motions) when the *velocities are nothing;* that is, when the *opposing forces are pressures,*" &c. Here Mr. T. plainly tells us when compared with what goes before, that two quiescent bodies which do not touch, or, if he will have it so, two bodies which do touch and are wholly destitute of any natural or impressed tendency to approach if they could, or to change their places, press each other! But the chief merit of this passage is not confined to this conclusion. It is manifest from the drift of it Mr. T. can compare pressure with impulse. Of course he can also compare a mathematical line with an area; and thence tell us how many lines there are in a superficies, how many superficies in a solid; and, as a finale, I expect how many inches in an hour.

Again, says Mr. T. "If two hard bodies moving in the same direction with different momenta, so that the body having the greater momentum strikes the other, the sum of the momenta before and after the stroke will be the same, but an exchange will take place; for after the stroke, the striking body will move with the momentum of the body struck."

Let A be the striking body, and a its velocity, B the other body, and b its velocity. By Mr. Tredgold's law, B b is the motion of A after the stroke; that is, the motion of A after the stroke = A a − (A a − B b); and so likewise the motion of B after the stroke = B b + (A a − B b). Therefore when A a = B b; that is, when the momenta before the collision are equal, or the velocities reciprocally proportional to the bodies, the motions, and of course the velocities, of the bodies are unaffected by the collision; and each body retains the same velocity after the collision it had before. But the velocity of A must have been greater than that of B before the collision, otherwise it could not have overtaken and struck it; consequently it must likewise be as much greater after collision. Now if one body overtake and strike another moving in the same right line, the striking body must after collision have a less or cannot have a greater velocity than the body struck, in consequence of its being obliged to move behind the other. But we have shown it is greater; and it may be as many times greater as we please to imagine B to be greater than A. Therefore the velocity of the striking body after collision is both *greater* and *not greater* than the velocity of the other. In the same way it may be shown that the velocity of the body struck will after collision be both *less* and *not less* than that of the other body.

A theory that admits such conclusions as these needs no comment on its merits.

At p. 133, Mr. T. observes: "If two hard bodies move in opposite directions upon the same line, with different momenta, the momentum after the stroke will be equal to the difference of the momenta before the stroke. The body which had the *greatest* momentum before the stroke *will be at rest after it*, and the other body will move with a momentum equal to the difference of the momenta before the stroke."

Here it is plain Mr. T. assumes the intensity of collision to be equal to the greater momentum; because if it was either less or greater, this body would after collision have some motion in the same or opposite direction. Therefore the opposite motion of the other body contributes nothing to the intensity of the stroke, which would be equally as great whether this body was at rest, or moving with a momentum equal and contrary to the other. Now I wish to put Mr. Tredgold to no inconvenience, but if he could get some one to stand still, while he walked at a certain rate up against him; and if he could then induce the other, instead of standing still, to meet him with an equal motion, I think he would have a feeling experimental proof of the falseness of his theory.

Unfortunately Mr. T. has not demonstrated this theorem; and I must acknowledge I cannot see how it is derived. There is also a difficulty in the theorem itself I am unable to comprehend. For instance, I have shown by this theorem that the less motion contributes nothing to the stroke; and this must hold good even when it is but ever so trifling less than the other motion. On the contrary, if it be increased to but ever so trifling a degree greater, it will contribute the whole of the stroke. Surely this is a very convenient transfer of power between inanimate bodies; but on what physical principles can it be explained? How does it operate in the case of equality of momenta? In which of the bodies does the power of giving intensity to collision then side? or how is it divided between them?

But in Case 2 of his last paper, which is precisely the present theorem, Mr. T. tells us that the "*deficiency of reaction is* A V − B v." Therefore as B v increases, this deficiency diminishes, and the reaction itself increases. But the reaction is only the counterpart and equivalent of the action; and the action is evidently the intensity of collision. The intensity of collision, therefore, increases by a quantity equal to the less motion as this less motion increases; and is the least when this motion is the least or nothing. When consequently the less motion becomes equal to A V the greater, the intensity of collision exceeds the intensity when $B v = o$ by A V. But Mr. T. acknowledges when $B v = o$ that the intensity is A V; therefore when $B v = A V$, the intensity is $A V + A V = 2 A V$, the same as in my theory. Thus had Mr. T. argued correctly from his assumption, he would, with the part he has, without acknowledgment, borrowed from my theory, have brought out the same as I had, the very conclusions he wants to refute. But to return: it is plain from Mr. T.'s views here expounded, that the intensity of collision is greater the greater $B v$ is; yet from what he says of the same thing in his first paper, I have shown that this intensity is invariably the same for every value of $B v$ less than A V. Therefore it is evident that Mr. Tredgold's notions of collision are such, that he cannot, under equal circumstances, at different times, take like views of the same thing.

Such is a fair representation of that part of Mr. Tredgold's theory which is due to his own invention. Other absurdities and strange paralogies I could easily adduce from Mr. T.'s theory of collision were I disposed; but I have passed them over that it might not be said I strive to overcharge the picture. What I have exhibited will be sufficient to show what confidence can be placed in the observations and discoveries of Mr. T.; and as he seems not less determined to try to refute my theory than ambitious to become the author of a new one; these examples of his success in that part of his theory which he has not taken from mine, will, if they cannot convince him, demonstrate to the world how well he can refute or discover.

I shall not now, because it is extraneous to the subject, stop to refute the absurd doctrine Mr. Tredgold would propagate in the last paragraph of his last letter, which, according to Mr. T.'s discoveries, would render all philosophers, except himself, little better than simpletons, and all works, except his own, visionary and false. For the same reason I shall not reply to the illiberal, unhandsome, and uncalled-for insinuation he has thought proper, in a postscript, to add to his last letter. There is, however, one thing too closely connected with the present subject to be passed over; and as it is perfectly gratuitous on his part, will, if not fulfilled, admit of but one construction. At the end of his first paper he says: "*I may also remark, that there is a much more simple and consistent manner of accounting for the greater part of the phænomena he (Mr. Herapath) has attempted to explain.*" Of course Mr. T. must be acquainted with this method, perfectly able to apply it, and thoroughly satisfied of its truth; otherwise he would not venture to speak so confidently of it. Now I should be sorry to take Mr. T. at a disadvantage, and, therefore, if in next month's number he will candidly acknowledge this to be a mistake, it may rest as it is. If he do not, he will have the goodness to observe that the assertion was totally unasked for, and irrelevant to the subject of his paper, the "refutation" of my theory of collision, or rather the publication of his own. I need not observe to Mr. T. that an assertion thus unnecessarily and gratuitously made on the works of another, in an attack of this kind, the world will expect him when called on to fulfil, or unequivocally to confess its inaccuracy. Should Mr. T. determine to support his assertion, and to try his skill at philosophizing, there will be no necessity for words; it will be best to proceed with the subject. Any time, I assure him, he shall have my consent to take for the accomplishment of his object that he pleases; and he shall likewise have his choice of any of the phænomena I have explained, provided, however, he take "*the greater part*," and those which involve mathematical laws and calculations, which are alone the true test of a theory. If Mr. T. will neither support, nor openly and candidly abandon his assertion, he must pardon me for declining to continue the discussion, or to notice any future observations he may please to make.

I am, dear Sir,
Your very obedient servant,
J. HERAPATH

ARTICLE X.

SCIENTIFIC INTELLIGENCE, AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Congreve Rockets.*

These destructive missiles have lately been employed with considerable effect in the whale fisheries. Capt. Scoresby, who is well known in the scientific world for his observations on the polar basin, was, we believe, the first to adopt this ingenious mode of capturing the "monarch of the ocean." The Fame has brought home nine fish, the whole of which were taken by this means. In one case, instant death was produced by a single rocket; and in all cases the speed of the fish was much diminished, and its power of sinking limited to three or four fathoms. The peculiar value and importance of the rocket in the fisheries is, that by means of it all the destructive effects of a six or even a twelve pounder piece of artillery, may be given with an apparatus not heavier than a musket, and with scarcely any shock or reaction on the boat. It appears that some of the smallest rockets employed in the Fame penetrated completely through the body of the fish, so that the effect of the explosion was visible on the opposite side. On the score of humanity, the employment of these rockets is also very desirable, as their fierce and destructive fire acting on the vitals of the animal, almost instantly destroys life; and saves the lingering tortures of the harpoon, axe, and even saw, which are occasionally resorted to.

II. *Improvement in Woulfe's Apparatus.*

The following improved form of Woulfe's apparatus is due to the Marquess Ridolfi. The bottles have three apertures as usual; and the middle one, intended for the purpose of cleansing the bottle, or the introduction of materials, is closed either by a cork or a stopper; tubes descend by the other two into the bottles, one a little way in, the other nearly to the bottom; these tubes are small; they are fastened into the neck of the bottle, and do not rise far above. Each of these is surrounded on the exterior of the bottle by a larger tube as high as themselves, and fastened by cement on the tops of the necks or tubulars, so as to form a little vessel to receive mercury round the outside of the smaller tube. The connexion is then easily made between one or more of these bottles by glass tubes bent twice at right angles, and of such size as easily to slip in between the two tubes before described. When the lower ends are immersed in the mercury, all is tight, and the apparatus may be set to work. This contrivance allows a little motion to the bottles without endangering them; they are instantly connected or unconnected at pleasure, and they act to a certain extent as tubes of safety.—(Journal of Science.)

III. *Yellow Fever.*

An English physician has lately published a letter against the precautionary measures adopted by the Spanish government to prevent contagion. He observes, that the fears which these measures excite, the want of care experienced by the sick who are abandoned, and the famine which follows the suspension of all communication with a town, are circumstances which extend the ravages of the fever to those who would otherwise have resisted the contagion. He advises the cleaning of the places where the disease prevails, and the allowing persons not affected to remove to healthy districts.

The following Prize Questions connected with this subject have lately been proposed by the Academic Society of Nantes:

"What are the origin, the causes, and the nature, of the yellow fever?"

"What are the means of preventing it?"

IV. *Ancient Boats.*

Two oak canoes of great antiquity have lately been discovered in Loughisland Ravey, near Castlewellan, Ireland. They were each 21 feet in length, and excavated from the tree; the whole of the wood being perfectly sound and in a high state of preservation. It appears that they were intended, when lashed together, to form a double boat, such as is now employed in the South Seas, the right side of the one, and the left side of the other being convex in form, while the two opposite sides were perfectly strait, so that they could very readily be joined together. Unfortunately the finders of these primeval relicts in a few hours destroyed what the lapse of ages had failed to effect.

V. *Steam Navigation.*

It is generally supposed that the application of the steam-engine to the purposes of navigation is of very recent invention: this, however, is not the case, as a patent was granted to Jonathan Hulls in 1736, in which it was expressly stated that he should employ it for the conveyance of vessels. The specification is placed at the beginning of a very rare tract preserved in the library of the British Museum. The above work is illustrated by an engraved plate, representing a steam-boat towing a large vessel, and contains the following account of the apparatus employed:

"In some convenient part of the tow-boat, there is placed a vessel, about two-thirds full of water, with the top close shut; this vessel being kept boiling rarefies the water into a steam; this steam being conveyed through a large pipe into a cylindrical vessel, and there condensed, makes a vacuum, which causes the weight of the atmosphere to press on this vessel, and so presses down a piston that is fitted into this cylindrical vessel in the same manner as in Mr. Newcomen's engine, with which he raises water by fire."

After Hulls, we may place M. Duquet, the Marquis de Jouffroy, and Lord Stanhope, as the next in the list of candidates for the honour of this invention, all of whom are prior to Fulton, the so much boasted transatlantic engineer. We have noticed this subject, as it serves to illustrate a part of the history of the most stupendous machine which science has yet presented to the aid of man; but a useful treatise on this subject has long been wanted, and we feel pleasure in stating that this important chasm in scientific literature is likely to be supplied by the immediate publication of a work on the subject by Mr. Partington, of the London Institution, an analysis of which we hope to lay before our readers in an early number of the *Annals*.

VI. *New Apple.*

A gardener resident at Barnwell, near Cambridge, has, after many years' trial, brought an entire new species of apple to very great perfection. It is admirably adapted for keeping; and for magnitude and weight it exceeds all that are known in this country, measuring twelve inches in diameter, while it exceeds one pound in weight.

VII. *Goodman's Improved Stirrup-Iron.*

This patent improvement consists in the introduction of a cross-bar, bearing a spring within the open bottom of a stirrup-iron supporting a false bottom, which rises and falls according to the motion of the horse, and affords relief to the rider; the horse is also relieved from any sudden pressure, and they prevent the breaking of the saddle-tree, the weight of the rider being uniformly carried upon an elastic instead of a solid bearing.

VIII. *Comets.*

It appears that the late Mr. Cusac has left some unpublished papers on comets. He supposes them to be globes of water; that, on return to perihelion, the solar rays (after sunset) strike on the mass of water, enter converging to the centre, where, after decussation, they emerge from the liquid globe diverging, and form the phenomenon in the heavens called the comet's tail. As to the use of these watery bodies, he thinks they were formed by nature to assist in giving a due temperature to our system.

IX. *North Polar Expedition.*

It appears from an account furnished by a gentleman connected with the expedition under the command of Capt. Frankland, that the party had arrived at about 64° N. lat. and 110 W. long. The whole expedition were in good health and spirits, and had passed an agreeable winter, living on the flesh of rein deer, which animal abounds in those regions, and passed them in great droves. The encampment was made in Sept. 1820, when further progress became impracticable, and in June of the present year, they were to commence their passage down Hearn's River. The party consisted of Capt. Franklin, two naval officers, one seamen, 19 Canadian *voyageurs*, and 17 Indians, making in all 40 persons.

Letters have also been received from the Discovery Ships, dated July 16; they were then at Resolution Island, in Hudson's Bay. They had met with some heavy icebergs, and considerable obstructions from the field ice, which was then rapidly disappearing. When, however, the accounts came away, they had surmounted these inconveniences, and were pursuing their voyage of discovery up the inlet at the north of the Bay. The officers and men were in the highest health and spirits, being well provided with all the necessary articles of provision and clothing adapted to the climate.

X. *Musical Permutation.*

A very curious invention has been made in the art of musical composition. It consists in the use of prepared cards, on each of which a bar of an air is arranged according to a certain rhythm and key. Four packs of these cards, marked A, B, C, and D, are mingled together, and as the cards are drawn and arranged before a performer in the order of that series, it will be found that an original air is obtained. The cards hitherto made are as waltzes, and succeed perfectly.

XI. *Singular Species of Strawberry.*

A correspondent states, that the singular species of strawberry lately noticed in the public papers, found in Scotland, and which, like the famous Glastonbury thorn, blooms in winter, is not confined to one part of our island, but has flourished upwards of 50 years in the garden of Tintern Abbey, the seat of Cæsar Colclough, Esq. It was transplanted thither from Holland by a Mr. Simon, who presented it to one of Mr. Colclough's ancestors.

XII. *Adulteration of Milk.*

Mr. E. Davy has lately completed a series of experiments on this subject. He states that the amount of adulteration even in skimmed milk sold in Cork, amounted to from one-fourth to one-sixth part. The worst of the adulterated milk from the market being of the specific gravity of 1·026, while the highest of the genuine milk from the market was 1·039. In no case, however, did it appear that either chalk, flour, or starch, was employed, the first being insoluble in skimmed milk, and as well as the flour and starch speedily subsided.

To ascertain the purity of new milk, it is only necessary to employ a glass tube or lactometer minutely graduated, and the proportion that the cream bears in point of depth to the milk beneath, marks the purity of the fluid operated upon. The lactometer employed by Mr. Davy, and with which he produced the above results, was little more than a common hydrometer graduated for the purpose, a thermometer being attached to mark the precise temperature at the time of making the experiment.

XIII. *Maturation of Fruits.*

M. Berard has been engaged in a course of experiments to determine what chemical changes take place during the maturation, ripening, and decay of fruits of various kinds: his general results are stated as follows; viz.

"Fruits act upon atmosperical air in a different manner to leaves. The former at all times, both in light and darkness, part with carbon to the oxygen of the atmosphere, to produce carbonic acid, and this loss of carbon is essential to ripening, since the process stops, if the fruit is immersed in an atmosphere deprived of oxygen, and the fruit itself shrivels and dies. This occurs equally to those fruits which, when gathered green, are able to ripen of themselves, though separated from their parent tree; but in these, the ripening process may be by this means delayed for a certain time, and be completed by restoring them to an oxygenized atmosphere. In this manner, peaches, plums, apples, pears, &c. may be preserved unspoiled from three to ten or twelve weeks, inclosed in an air-tight jar, with a quantity of lime and sulphate of iron worked up into a paste with water, which has the property of abstracting oxygen from the air which is in contact with it. The passing from ripeness to decay in fruits is also characterized by the production and evolution of much carbonic acid, and equally requires the presence of an oxygenized medium. The internal changes produced in fruits by the ripening process are particularly distinguished by the production of sugar, which does not exist in any considerable quantity in immature fruits; and it appears to be produced at the expense of part of the gum, and especially of the ligneous fibre."—(Phil. Mag.)

ARTICLE XI.

NEW SCIENTIFIC BOOKS

PREPARING FOR PUBLICATION.

A Translation of Baron Laney's New Work, entitled, "A Collection of Surgical Observations," with Notes. By R. Daughson.

An Appendix to Prof. Orfila's General System of Toxicology.

The Works and Life of John Playfair, FRS. &c. 4 Vols. 8vo.

Reflections on Gall and Spurzheim's System of Physiognomy and Phrenology. By John Abernethy, FRS, 8vo.

JUST PUBLISHED.

Allan's System of Pathological and Operative Surgery, founded on Anatomy. Vol. II. 12*s.* 6*d.*

Observations on Female Diseases. By Dr. Clarke. Part II. Royal 8vo. 15*s.*

The Parent's Medical and Surgical Assistant. By Thomas Ayre Bromhead, MB. 12mo. 4*s.*

Zoological Illustrations, or Original Figures, and Descriptions of new, rare, or interesting Animals. By William Swainson, FRS. Vol. I. 2*l.* 18*s.* 6*d.*

A Natural History of the Crinoidea, or Lily-shaped Animals. By J. S. Miller, ALS. 4to. 2*l.* 12*s.* 6*d.*

The Voyager's Companion, or Shell Collector's Pilot. By John Mawe. 18mo. 5*s.*

A Natural Arrangement of British Plants according to their Relations to each other, as pointed out by Jussieu, De Candolle, Brown, &c. including those cultivated for Use, with the Characters, Differences, Synonyms, Places of Growth, Time of Flowering, and Sketch of their Uses, with an Introduction to Botany, in which the Terms newly introduced are explained. By Samuel Frederick Gray, Lecturer on Botany, &c. with 21 Plates, in Two very large Volumes, 8vo. 2*l.* 2*s.*

ARTICLE XII.

NEW PATENTS.

John Collinge, of Lambeth, Surrey, engineer; for an improvement on cast-iron rollers for sugar-mills, by more permanently fixing them to their gudgeons.—Aug. 14, 1821.

John Nichol, of West-End, St. John's, Hampstead, Middlesex, master-mariner; for an improved capstan, windlass, and hawse-roller. —Aug. 22.

Sir William Congreve, of Cecil-street, Strand, Middlesex, Bart.; for certain improvements on his former patent, bearing date Oct. 19, 1818, for certain new methods of constructing steam-engines.—Sept. 28.

James Fergusson, of Newman-street, Oxford-street, Middlesex, stereotyper and printer; for improvements upon, additions to, or substitutes for, certain materials or apparatus made use of in the process of printing from stereotype plates.—Oct. 18.

Stephen Hawkins, of the Strand, Middlesex, civil engineer; for certain improvements on air-traps for privies, water-closets, close-stools, and chamber conveniences, to which the same may be applicable.—Oct. 18.

Thomas Lees, jun. of Birmingham, snuffer-manufacturer; for certain improvements in the construction of snuffers.—Oct. 18.

Peter Davey, of Old Swan-wharf, Chelsea, Middlesex, coal-merchant; for an improved preparation of coal for fuel.—Oct. 18.

John Poole, of Sheffield, victualler; for certain improvements in plating iron or steel with brass or copper, or copper allayed with other metal or metals, both plain and ornamental, for the purpose of rolling and working into plates, sheets, or bars; and such goods or wares to which the same may be found applicable.—Oct. 18.

John Christophers, of New Broad-street, London; for certain improvements on, or a substitute for, anchors.—Oct. 18.

ARTICLE XIII.

METEOROLOGICAL TABLE.

1821.	Wind.	BAROMETER.		THERMOMETER.		Evap.	Rain.	Hygr. at 9 a.m.	
		Max.	Min.	Max.	Min.				
10th Mon.									
Oct. 1	N W	30·12	29·75	65	43	—			
2	N W	30·18	30·04	61	44	—			
3	S W	30·04	29·71	68	60	—	26		
4	S	29·89	29·58	65	43	—	91		☾
5	N W	30·10	29·89	58	43	47			
6	W	30·12	30·10	65	53	—			
7	S W	30·10	30·00	67	51	—	05		
8	N W	30·33	30·10	63	33	—	12		
9	S W	30·33	30·14	60	35	—			
10	S E	30·14	29·81	61	41	35			
11	S E	29·85	29·75	58	46	—	—		○
12	N W	30·36	29·85	58	39	—			
13	N	30·40	30·36	60	33	—			
14	S E	30·40	30·32	61	46	—	18		
15	N E	30·32	30·24	54	32	—	03		
16	N	30·24	30·24	58	36	—			
17	N W	30·24	30·02	61	45	—			
18	S W	30·02	29·98	57	45	35	—		☽
19	N W	29·98	29·32	66	46	—			
20	S W	29·29	29·08	57	40	—	26		
21	N W	29·29	29·29	52	46	—	27		
22	S W	29·29	29·29	56	42	—	04		
23	S E	29·45	29·42	53	35	—	15		
24	S W	29·52	29·52	51	38	—	01		
25	S W	30·03	30·03	53	42	—	03		
26	S W	30·13	30·13	58	50	—			●
27	S W	30·22	30·22	61	54	—	12		
28	S W	30·30	30·30	60	51	—	—		
29	S E	30·32	30·18	51	34	—			
30	E	30·18	30·00	57	37	—			
31	N E	30·00	30·00	60	46	46	08		
		30·40	29·08	68	31	1·63	2·51		

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

REMARKS.

Tenth Month.—1. Fine: windy. 2. Fine. 3. Cloudy. 4. Very rainy day: stormy night. 5, 6, 7. Fine. 8. Rainy morning: a *Stratus* on the marshes at night. 9. Foggy morning: fine: lunar corona at night. 10. Fine. 11. Cloudy. 12, 13, 14. Fine. 15. Morning, rainy: afternoon, fine. 16. Fine: *Stratus* at night. 17. Fine: fog at night. 18, 19. Cloudy. 20. Rainy. 21. Fine day: rainy night. 22. Cloudy: fine. 23. Rainy. 24. Cloudy. 25. Cloudy: foggy. 26. Cloudy. 27. Cloudy. 28. Foggy: drizzling: night clear: a *Stratus* on the marshes. 29. Foggy. 30. Fine: a *Stratus* at night. 31. Fine.

RESULTS.

Winds: N, 2; NE, 2; E, 1; SE, 5; S, 1; SW, 11; W, 1; NW, 8.

Barometer: Mean height	
For the month	29·981 inches.
For the lunar period, ending the 18th	29·990
For 14 days, ending the 9th (moon south)	29·957
For 13 days, ending the 22d (moon north)	29·905
Thermometer: Mean height	
For the month	50·709°
For the lunar period	54·133
For 30 days, the sun in Libra	53·016
Evaporation	1·63 in.
Rain	2·51

Laboratory, Stratford, Eleventh Month, 21, 1821.

R. HOWARD.

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THE END.

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